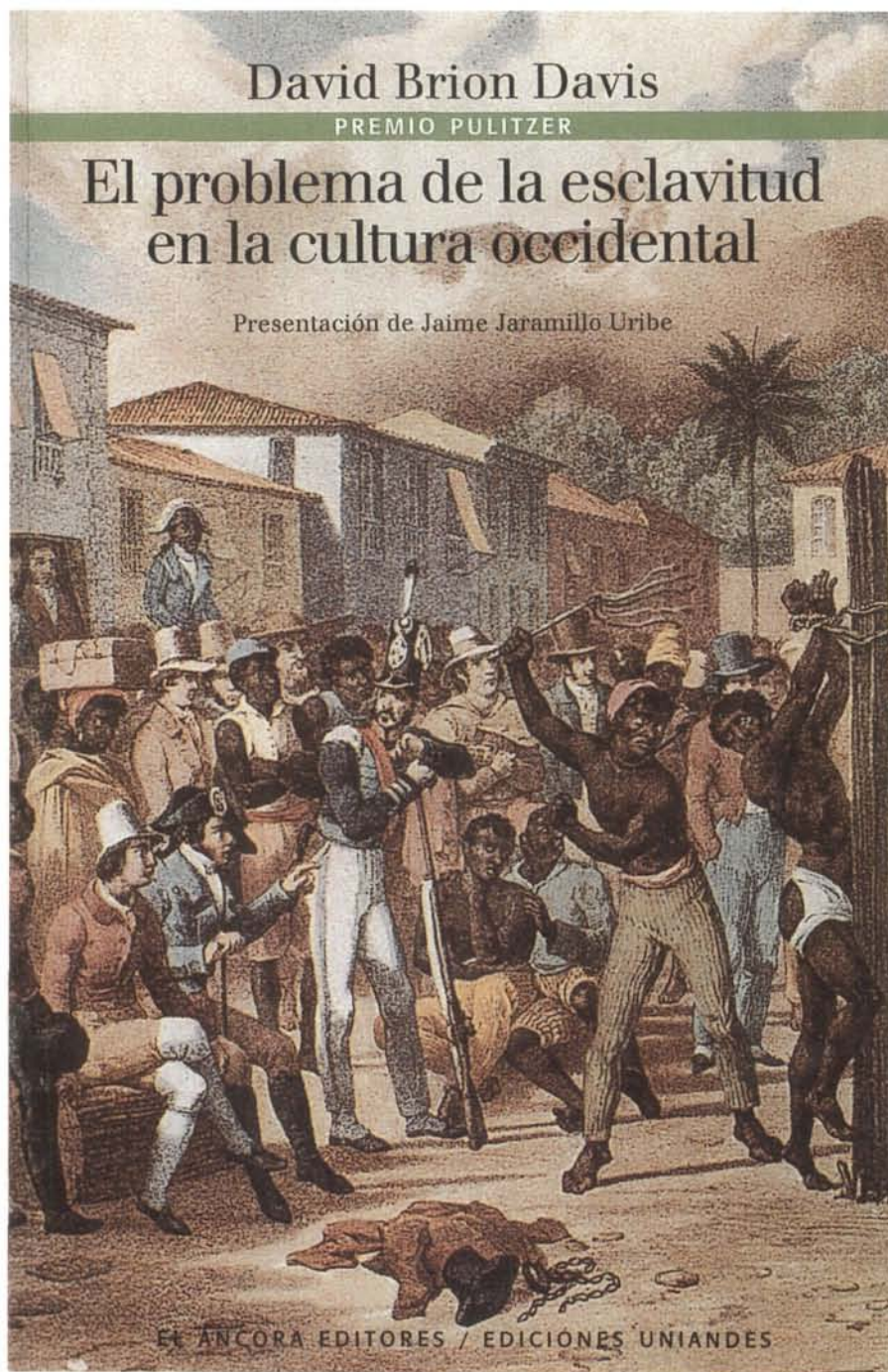


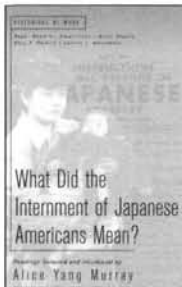
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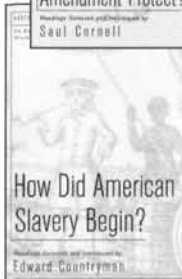
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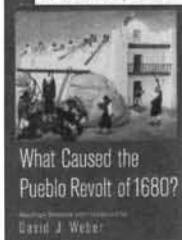
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Contents

VOLUME 105 • NUMBER 2 • APRIL 2000

In This Issue

xiii

Articles

Honor, Masculinity, and Ritual Knife Fighting in Nineteenth-Century Greece
BY THOMAS W. GALLANT 359

Modernization from the Other Shore: American Observers and the Costs of
Soviet Economic Development
BY DAVID C. ENGERMAN 383

A Time of Reconquest: History, the Maya Revival, and the Zapatista Rebellion
in Chiapas
BY THOMAS BENJAMIN 417

AHR Forum: Crossing Slavery's Boundaries

INTRODUCTION 451

Looking at Slavery from Broader Perspectives
BY DAVID BRION DAVIS 452

The Big Picture: A Comment on David Brion Davis's "Looking at Slavery from
Broader Perspectives"
BY PETER KOLCHIN 467

Small-Scale Dynamics of Large-Scale Processes
BY REBECCA J. SCOTT 472

Slavery at Different Times and Places
BY STANLEY L. ENGERMAN 480

Review Essays: Counting and Power

INTRODUCTION	485
The Great Explanandum BY ROGER HART	486
Thinking Unfashionable Thoughts, Asking Unfashionable Questions BY MARGARET C. JACOB	494
Whose Measure of Reality? BY JACK A. GOLDSTONE	501

Reviews of Books

METHODS/THEORY

- | | |
|--|--|
| <p>JO MURPHY-LAWLESS. <i>Reading Birth and Death: A History of Obstetric Thinking.</i>
By Wendy Mitchinson 509</p> <p>JOHN C. BURNHAM. <i>How the Idea of Profession Changed the Writing of Medical History.</i>
By Daniel M. Fox 510</p> <p>JOHN T. GRAHAM. <i>Theory of History in Ortega y Gasset: "The Dawn of Historical Reason"</i>
By Ewa Domańska 510</p> <p>ROY ROSENZWEIG and DAVID THELEN. <i>The Presence of the Past: Popular Uses of History in American Life.</i>
By Robert R. Archibald 511</p> <p>DAVID HAMER. <i>History in Urban Places: The Historic Districts of the United States.</i>
By Thomas J. Noel 512</p> <p>GILBERT M. JOSEPH, CATHERINE C. LEGRAND, and RICARDO D. SALVATORE, editors. <i>Close Encounters of Empire: Writing the Cultural History of U.S.-Latin American Relations.</i>
By Frank Ninkovich 513</p> <p>DONALD F. STEVENS, editor. <i>Based on a True Story: Latin American History at the Movies.</i>
By Eric Zolov 515</p> <p>MICHEL LE GALL and KENNETH PERKINS, editors. <i>The Maghrib in Question: Essays in History and Historiography.</i>
By Allan Christelow 516</p> | <p>TERENCE C. HALLIDAY and LUCIEN KARPIK, editors. <i>Lawyers and the Rise of Western Political Liberalism: Europe and North America from the Eighteenth to Twentieth Centuries.</i>
By Kermit L. Hall 519</p> <p>DANIELA SPENSER. <i>The Impossible Triangle: Mexico, Soviet Russia, and the United States in the 1920s.</i>
By William Richardson 520</p> <p>NICHOLAS TARLING. <i>Britain, Southeast Asia and the Onset of the Cold War, 1945-1950.</i>
By John Kent 521</p> <p>DEBORAH WELCH LARSON. <i>Anatomy of Mistrust: U.S.-Soviet Relations during the Cold War.</i>
By Randall B. Woods 522</p> |
|--|--|

ASIA

COMPARATIVE/WORLD

- | | |
|--|---|
| <p>CONSTANCE CLASSEN. <i>The Color of Angels: Cosmology, Gender and the Aesthetic Imagination.</i>
By W. F. Bynum 518</p> <p>SANDER GILMAN. <i>Creating Beauty to Cure the Soul: Race and Psychology in the Shaping of Aesthetic Surgery.</i>
By Joel T. Braslow 518</p> | <p>JONATHAN D. SPENCE. <i>The Chan's Great Continent: China in Western Minds.</i>
By Soren Clausen 523</p> <p>ANGELA ZITO. <i>Of Body and Brush: Grand Sacrifice as Text/Performance in Eighteenth-Century China.</i>
By Susan Brownell 523</p> <p>ANNE WALTHALL. <i>The Weak Body of a Useless Woman: Matsuo Taseko and the Meiji Restoration.</i>
By Sharon Sievers 524</p> <p>VERA MACKIE. <i>Creating Socialist Women in Japan: Gender, Labour and Activism, 1900-1937.</i>
By Sharon A. Minichiello 525</p> <p>PAUL H. KRATOSKA. <i>The Japanese Occupation of Malaya: A Social and Economic History.</i>
By Alfred W. McCoy 526</p> <p>SANDRA C. TAYLOR. <i>Vietnamese Women at War: Fighting for Ho Chi Minh and the Revolution.</i>
By David G. Marr 527</p> <p>ANTONY COPLEY. <i>Religions in Conflict: Ideology, Cultural Contact and Conversion in Late-Colonial India.</i>
By Gauri Viswanathan 527</p> <p>ANAND A. YANG. <i>Bazaar India: Markets, Society, and the Colonial State in Gangetic Bihar.</i>
By Ian J. Kerr 529</p> |
|--|---|

CANADA AND THE UNITED STATES

- MARC EGNAL. *New World Economies: The Growth of the Thirteen Colonies and Early Canada.*
By Sean T. Cadigan 530
- JULIAN GWYN. *Excessive Expectations: Maritime Commerce and the Economic Development of Nova Scotia, 1740-1870.*
By Gordon T. Stewart 531
- E. A. HEAMAN. *The Inglorious Arts of Peace: Exhibitions in Canadian Society during the Nineteenth Century.*
By A. A. den Otter 532
- NINETTE KELLEY and MICHAEL TREBILCOCK. *The Making of the Mosaic: A History of Canadian Immigration Policy.*
By Carmela Patrias 532
- MARY-ELLEN KELM. *Colonizing Bodies: Aboriginal Health and Healing in British Columbia, 1900-50.*
By Christopher Bracken 533
- MARILYN C. BASELER. "Asylum for Mankind": *America, 1607-1800*; MARIANNE S. WOKECK. *Trade in Strangers: The Beginnings of Mass Migration to North America.*
By Virginia DeJohn Anderson 534
- EDWIN G. BURROWS and MIKE WALLACE. *Gotham: A History of New York City to 1898.*
By David C. Hammack 536
- LISA WILSON. *Ye Heart of a Man: The Domestic Life of Men in Colonial New England.*
By Elizabeth Reis 537
- CYNTHIA A. KIERNER. *Beyond the Household: Women's Place in the Early South, 1700-1835.*
By Janet L. Coryell 538
- JOHN GILMAN KOLP. *Gentlemen and Freeholders: Electoral Politics in Colonial Virginia.*
By Warren R. Hofstra 539
- RICHARD BROOKHISER. *Alexander Hamilton, American*; ARNOLD A. ROGOW. *A Fatal Friendship: Alexander Hamilton and Aaron Burr.*
By Richard K. Matthews 539
- ANYA JABOUR. *Marriage in the Early Republic: Elizabeth and William Wirt and the Companionate Ideal.*
By Glenna Matthews 541
- JOHN H. WIGGER. *Taking Heaven by Storm: Methodism and the Rise of Popular Christianity in America.*
By Randy J. Sparks 541
- CARL BENN. *The Iroquois in the War of 1812.*
By Cornelius J. Jaenen 542
- KEITH R. WIDDER. *Battle for the Soul: Métis Children Encounter Evangelical Protestants at Mackinaw Mission, 1823-1837.*
By Jennifer S. H. Brown 543
- ALEXANDRA HARMON. *Indians in the Making: Ethnic Relations and Indian Identities around Puget Sound.*
By Clifford E. Trafzer 544
- HARLOW W. SHEIDLEY. *Sectional Nationalism: Massachusetts Conservative Leaders and the Transformation of America, 1815-1836.*
By Ronald P. Formisano 545
- EDWARD A. PEARSON, editor. *Designs against Charleston: The Trial Record of the Denmark Vesey Slave Conspiracy of 1822.*
By Winthrop D. Jordan 546
- STEVEN WEISENBURGER. *Modern Medea: A Family Story of Slavery and Child-Murder from the Old South.*
By Carol Wilson 547
- JEANIE ATTIE. *Patriotic Toil: Northern Women and the American Civil War.*
By Nina Silber 547
- WARREN B. ARMSTRONG. *For Courageous Fighting and Confident Dying: Union Champlains in the Civil War.*
By Michael Barton 548
- DENNIS J. RINGLE. *Life in Mr. Lincoln's Navy.*
By Robert M. Browning, Jr. 549
- KATHLEEN WATERS SANDER. *The Business of Charity: The Women's Exchange Movement, 1832-1900.*
By Patricia A. Cooper 550
- GILLIAN GILL. *Mary Baker Eddy.*
By Mary Farrell Bednarowski 551
- JONATHAN ZIMMERMAN. *Distilling Democracy: Alcohol Education in America's Public Schools, 1880-1925.*
By Jack S. Blocker, Jr. 551
- LEONARD WARREN. *Joseph Leidy: The Last Man Who Knew Everything.*
By John S. Haller, Jr. 552
- STEVEN CONN. *Museums and American Intellectual Life, 1876-1926.*
By Robert C. Bannister 553
- DAVID S. CECELSKI and TIMOTHY B. TYSON, editors. *Democracy Betrayed: The Wilmington Race Riot of 1898 and Its Legacy.*
By Peter W. Bardaglio 554
- WINSTON JAMES. *Holding Aloft the Banner of Ethiopia: Caribbean Radicalism in Early Twentieth-Century America.*
By Brenda Gayle Plummer 555
- JAMES J. CONNOLLY. *The Triumph of Ethnic Progressivism: Urban Political Culture in Boston, 1900-1925.*
By Steven P. Erie 556
- AMY BRIDGES. *Morning Glories: Municipal Reform in the Southwest.*
By Carl Abbott 557
- THAD SITTON and JAMES H. CONRAD. *Nameless Towns: Texas Sawmill Communities, 1880-1942.*
By Richard W. Judd 558
- JOHN C. HENNEN. *The Americanization of West Virginia: Creating a Modern Industrial State, 1916-1925.*
By Cecilia Elizabeth O'Leary 558
- YUVAL P. YONAY. *The Struggle over the Soul of Economics: Institutional and Neoclassical Economists in America Between the Wars.*
By David Felix 559
- JEFFREY HAYDU. *Making American Industry Safe for Democracy: Comparative Perspectives on the State and Employee Representation in the Era of World War II*; JOSEPH A. MCCARTIN. *Labor's Great War: The Struggle for Industrial Democracy and the Origins of Modern American Labor Relations, 1912-1921.*
By Ken Fones-Wolf 560
- DAVID M. HART. *Forged Consensus: Science, Technology, and Economic Policy in the United States, 1921-1953.*
By Jeffrey K. Stine 562
- ROBERT G. SPINNEY. *World War II in Nashville: Transformation of the Homefront.*
By Durwood Dunn 562

AMY BENTLEY, <i>Eating for Victory: Food Rationing and the Politics of Domesticity</i> . By Susan M. Hartmann	563	MARTHA K. HUGGINS, <i>Political Policing: The United States and Latin America</i> . By Robert H. Holden	581
DENNIS J. DUNN, <i>Caught Between Roosevelt and Stalin: America's Ambassadors to Moscow</i> . By Thomas R. Maddux	564	LUIS MARTÍNEZ-FERNÁNDEZ, <i>Fighting Slavery in the Caribbean: The Life and Times of a British Family in Nineteenth-Century Havana</i> . By Darien J. Davis	582
BRUCE E. FIELD, <i>Harvest of Dissent: The National Farmers Union and the Early Cold War</i> . By R. Douglas Hurt	565	JOAN CASANOVAS, <i>Bread, or Bullets! Urban Labor and Spanish Colonialism in Cuba, 1850–1898</i> . By Philip A. Howard	583
JEFF LAND, <i>Active Radio: Pacifica's Brash Experiment</i> ; MATTHEW LASAR, <i>Pacifica Radio: The Rise of an Alternative Network</i> . By Gary D. Rawnsley	566	G. POPE ATKINS and LARMAN C. WILSON, <i>The Dominican Republic and the United States: From Imperialism to Transnationalism</i> . By Michael J. Kryzanek	584
NANCY SHOEMAKER, <i>American Indian Population Recovery in the Twentieth Century</i> . By Thomas A. Britten	567	CARL C. CAMPBELL, <i>The Young Colonials: A Social History of Education in Trinidad and Tobago, 1834–1939</i> . By Alvin Magid	585
DAVID M. REIMERS, <i>Unwelcome Strangers: American Identity and the Turn Against Immigration</i> . By Gary Y. Okihiro	568	MATTHEW RESTALL, <i>Maya Conquistador</i> . By Robert W. Patch	586
WENDY F. KATKIN, NED LANDSMAN, and ANDREA TYREE, editors, <i>Beyond Pluralism: The Conception of Groups and Group Identities in America</i> . By David M. Reimers	569	TIMOTHY J. HENDERSON, <i>The Worm in the Wheat: Rosalie Evans and Agrarian Struggle in the Puebla-Tlaxcala Valley of Mexico, 1906–1927</i> . By Heather Fowler-Salamini	586
ROBERT C. LIEBERMAN, <i>Shifting the Color Line: Race and the American Welfare State</i> ; MICHAEL K. BROWN, <i>Race, Money, and the American Welfare State</i> . By Michael B. Katz	570	ANNE RUBENSTEIN, <i>Bad Language, Naked Ladies, and Other Threats to the Nation: A Political History of Comic Books in Mexico</i> . By Mauricio Tenorio	588
J. MORGAN KOUSSER, <i>Colorblind Injustice: Minority Voting Rights and the Undoing of the Second Reconstruction</i> . By Mark Tushnet	572	VIRGINIA GARRARD-BURNETT, <i>Protestantism in Guatemala: Living in the New Jerusalem</i> . By David McCreery	589
GEORGE LIPSITZ, <i>The Possessive Investment in Whiteness: How White People Profit from Identity Politics</i> . By Matthew Frye Jacobson	573	JAMES HOWE, <i>A People Who Would Not Kneel: Panama, the United States and the San Blas Kuna</i> . By Joseph L. Arbena	590
ALAN DERICKSON, <i>Black Lung: Anatomy of a Public Health Disaster</i> . By Claudia Clark	573	MARÍA ROSTWOROWSKI DE DIEZ CANSECO, <i>History of the Inca Realm</i> . By Alan L. Kolata	591
DANIEL HOROWITZ, <i>Betty Friedan and the Making of the Feminine Mystique: The American Left, the Cold War, and Modern Feminism</i> . By Annelise Orleck	574	MICHAEL EDWARD STANFIELD, <i>Red Rubber, Bleeding Trees: Violence, Slavery, and Empire in Northwest Amazonia, 1850–1933</i> . By David Sowell	591
LEILA J. RUPP, <i>A Desired Past: A Short History of Same-Sex Love in America</i> ; LILLIAN FADERMAN, <i>To Believe in Women: What Lesbians Have Done for America—A History</i> . By Martha Vicinus	575	THOMAS M. COHEN, <i>The Fire of Tongues: Antônio Vieira and the Missionary Church in Brazil and Portugal</i> . By Stuart B. Schwartz	592
JERRY LEMBCKE, <i>The Spitting Image: Myth, Memory, and the Legacy of Vietnam</i> . By Marita Sturken	577		
GORDER SILVERSTEIN, <i>Imbalance of Powers: Constitutional Interpretation and the Making of American Foreign Policy</i> . By James C. Schneider	577	EUROPE: ANCIENT AND MEDIEVAL	
ANNE HESSING CAHN, <i>Killing Detente: The Right Attacks the CIA</i> . By Rhodri Jeffreys-Jones	578	ERICH S. GRUEN, <i>Heritage and Hellenism: The Reinvention of Jewish Tradition</i> . By Calum Carmichael	593
MARY PATRICE ERDMANS, <i>Opposite Poles: Immigrants and Ethnicity in Polish Chicago, 1976–1990</i> . By Joseph John Parot	579	MICHAEL RICHTER, <i>Ireland and Her Neighbours in the Seventh Century</i> . By Brendan Smith	594
		RICHARD ABELS, <i>Alfred the Great: War, Kingship and Culture in Anglo-Saxon England</i> . By Barbara Yorke	595
CARIBBEAN AND LATIN AMERICA		DONALD J. KAGAY and L. J. VILLALON, <i>The Final Argument: The Imprint of Violence on Society in Late Medieval and Early Modern Europe</i> ; GUY HALSALL, editor, <i>Violence and Society in the Early Medieval West</i> . By James Given	595
RENÉ DE LA PEDRAJA, <i>Oil and Coffee: Latin American Merchant Shipping from the Imperial Era to the 1950s</i> . By David Tengwall	580	JOHN FRANCE, <i>Western Warfare in the Age of the Crusades 1000–1300</i> . By A. J. Forey	596

ALEXANDER MURRAY. <i>Suicide in the Middle Ages. Volume 1, The Violent Against Themselves.</i> By Michael Goodich	597	SUSAN PLANN. <i>A Silent Minority: Deaf Education in Spain, 1550–1835.</i> By Carol A. Padden	616
JEAN-CLAUDE SCHMITT. <i>Ghosts in the Middle Ages: The Living and the Dead in Medieval Society.</i> By Allen J. Frantzen	598	TIMOTHY MITCHELL. <i>Betrayal of the Innocents: Desire, Power, and the Catholic Church in Spain.</i> By David D. Gilmore	617
KATHLEEN CUSHING. <i>Papacy and Law in the Gregorian Revolution: The Canonistic Work of Anselm of Lucca.</i> By Maureen C. Miller	599	JAVIER UGARTE TELLERÍA. <i>La nueva Covadonga insurgente: Orígenes sociales y culturales de la sublevación de 1936 en Navarra y el País Vasco.</i> By Pamela Beth Radcliff	618
M. T. CLANCHY. <i>Abelard: A Medieval Life.</i> By Norman F. Cantor	600	GEORGE HUPPERT. <i>The Style of Paris: Renaissance Origins of the French Enlightenment.</i> By Zachary S. Schiffman	619
BERNARD F. REILLY. <i>The Kingdom of León-Castilla under King Alfonso VII 1126–1157.</i> By James F. Powers	601	JOHN A. LYNN. <i>Giant of the Grand Siècle: The French Army, 1610–1715.</i> By Frederick C. Schneid	620
FIONA J. WATSON. <i>Under the Hammer: Edward I and Scotland, 1286–1307.</i> By Benjamin Hudson	602	GILLES POSTEL-VINAY. <i>La terre et l'argent: L'agriculture et le crédit en France du XVIII^e au début du XX^e siècle.</i> Roger Price	621
JOSEPH ZIEGLER. <i>Medicine and Religion c. 1300: The Case of Arnau de Vilanova.</i> By Nancy G. Siraisi	603	PAUL METZNER. <i>Crescendo of the Virtuoso: Spectacle, Skill, and Self-Promotion in Paris during the Age of Revolution.</i> By David P. Jordan	622
EUROPE: EARLY MODERN AND MODERN		DEAN DE LA MOTTE and JEANNENE PRZYBLYSKI, editors. <i>Making the News: Modernity and the Mass Press in Nineteenth-Century France.</i> By Matt K. Matsuda	623
ROBERT B. EDGERTON. <i>Death or Glory: The Legacy of the Crimean War.</i> By Andrew Lambert	604	DANIEL MOLLENHAUER. <i>Auf der Suche nach der "wahren Republik": Die französischen "radicaux" in der frühen Dritten Republik (1870–1890).</i> By John Rothney	624
AVIEL ROSHWALD and RICHARD STITES, editors. <i>European Culture in the Great War: The Arts, Entertainment, and Propaganda, 1914–1918.</i> By Modris Eksteins	604	ARNE JANSSON. <i>From Swords to Sorrow: Homicide and Suicide in Early Modern Stockholm.</i> By Pieter Spierenburg	625
LAWRENCE S. KAPLAN. <i>The Long Entanglement: NATO's First Fifty Years.</i> By John Baylis	606	RISTO ALAPURO. <i>Suomen synty: Paikallisenä ilmiönä 1890–1933.</i> [The Birth of Finland as a Local Phenomenon, 1890–193] By Niilo Kauppi	626
N. A. M. RODGER. <i>The Safeguard of the Sea: A Naval History of Britain, 660–1649.</i> By Robert C. Ritchie	607	PETER R. PRIESTER. <i>Geschiedenis van de Zeeuwse landbouw circa 1600–1910.</i> By J. L. Price	627
JAMIE CAMERON. <i>James V: The Personal Rule 1528–1542.</i> By Edward M. Furgol	607	ANDREAS EDEL. <i>Der Kaiser und Kurpfalz: Eine Studie zu den Grundelementen politischen Handelns bei Maximilian II. (1564–1576).</i> By Paula Sutter Fichtner	628
ANNA BRYSON. <i>From Courtesy to Civility: Changing Codes of Conduct in Early Modern England.</i> By Michael F. Graham	608	HANS MEDICK. <i>Weben und Überleben in Laichingen 1650–1900: Lokalgeschichte als allgemeine Geschichte.</i> By Terence McIntosh	629
ROY PORTER and G. S. ROUSSEAU. <i>Gout: The Patrician Malady.</i> By Edward Shorter	609	MICHAELA HOHKAMP. <i>Herrschaft in der Herrschaft: Die vorderösterreichische Obervogtei Triberg von 1737 bis 1780.</i> By David M. Luebke	630
RICHARD SHANNON. <i>Gladstone. Volume 2, 1865–1898.</i> By Travis L. Crosby	610	JAMES M. BROPHY. <i>Capitalism, Politics, and Railroads in Prussia, 1830–1870.</i> By Colleen A. Dunlavy	631
JOHN T. SMITH. <i>Methodism and Education, 1849–1902: J. H. Rigg, Romanism, and Wesleyan Schools.</i> By R. W. Ambler	611	JAMES H. JACKSON, JR. <i>Migration and Urbanization in the Ruhr Valley 1821–1914.</i> By Andrew Lees	632
BARBARA T. GATES. <i>Kindred Nature: Victorian and Edwardian Women Embrace the Living World.</i> By Paul Lawrence Farber	612	HANS-PETER GOLDBERG. <i>Bismarck und seine Gegner: Die politische Rhetorik im kaiserlichen Reichstag.</i> By Jonathan Sperber	633
DAVID CANNADINE. <i>The Rise and Fall of Class in Britain.</i> By J. V. Beckett	612		
ROSS MCKIBBIN. <i>Class and Cultures in England, 1918–1951.</i> By Harold L. Smith	613		
SEAN O'CONNELL. <i>The Car and British Society: Class, Gender and Motoring, 1896–1939.</i> By Peter Bailey	614		
JOHN KENDLE. <i>Federal Britain: A History.</i> By Bernard Crick	615		

- HEIKE FRANZ. *Zwischen Markt und Profession: Betriebswirte in Deutschland im Spannungsfeld von Bildungs- und Wirtschaftsbürgertum (1900–1945)*.
By Geoffrey Cocks 634
- MARTIN H. GEYER. *Verkehrte Welt: Revolution, Inflation und Modern München 1914–1924*.
By David Clay Large 635
- ODED HEILBRONNER. *Catholicism, Political Culture, and the Countryside: A Social History of the Nazi Party in South Germany*.
By Johnpeter Horst Grill 636
- NANCY CARTWRIGHT, ET AL. *Otto Neurath: Philosophy between Science and Politics*.
By David A. Hollinger 637
- PAUL LAWRENCE ROSE. *Heisenberg and the Nazi Atomic Bomb Project: A Study in German Culture*.
By J. L. Heilbron 638
- PATRICK MAJOR. *The Death of the KPD: Communism and Anti-Communism in West Germany, 1945–1956*.
By William A. Pelz 639
- MASSIMO FIRPO. *Dal Sacco di Roma all'Inquisizione: Studi su Juan de Valdés e la Riforma italiana*.
By Anne Jacobson Schutte 639
- CARLA FORTI. *Il caso Pardo Roques: Un eccidio del 1944 tra memoria e oblio*.
By Susan Zuccotti 640
- JERZY LUKOWSKI. *The Partitions of Poland: 1772, 1793, 1795*.
By Daniel Stone 641
- MARTA A. BALIŃSKA. *For the Good of Humanity: Ludwik Rajchman, Medical Statesman*.
By John F. Hutchinson 642
- MARTIN MALIA. *Russia under Western Eyes: From the Bronze Horseman to the Lenin Mausoleum*.
By Esther Kingston-Mann 643
- P. A. KROTOV. *Gangutskaia Bataliia 1714 goda*.
By Richard Hellie 644
- THOMAS M. BARRETT. *At the Edge of Empire: The Terek Cossacks and the North Caucasus Frontier, 1700–1860*.
By Philip Longworth 644
- DAVID ALAN RICH. *The Tsar's Colonels: Professionalism, Strategy, and Subversion in Late Imperial Russia*.
By Jonathan W. Daly 645
- OLEG KHARKHORDIN. *The Collective and the Individual in Russia: A Study of Practices*.
By Douglas R. Weiner 646
- R. W. DAVIES. *The Industrialisation of Soviet Russia. Volume 4, Crisis and Progress in the Soviet Economy, 1931–1933*.
By J. N. Westwood 647
- YITZHAK M. BRUDNY. *Reinventing Russia: Russian Nationalism and the Soviet State, 1953–1991*.
By Martin McCauley 648
- DOUGLAS R. WEINER. *A Little Corner of Freedom: Russian Nature Protection from Stalin to Gorbachev*.
By Paul R. Josephson 649
- MIDDLE EAST AND NORTHERN AFRICA
- NATHAN J. BROWN. *The Rule of Law in the Arab World: Courts in Egypt and the Gulf*.
By Peter Gran 650
- JAMES L. GELVIN. *Divided Loyalties: Nationalism and Mass Politics in Syria at the Close of Empire*.
By Bruce Masters 651
- CYRUS GHANI. *Iran and the Rise of Reza Shah: From Qajar Collapse to Pahlavi Rule*.
By Ervand Abrahamian 652
- SUB-SAHARAN AFRICA
- ERIC SILLA. *People are not the Same: Leprosy and Identity in Twentieth-Century Mali*.
By Andrew F. Clark 653
- RALPH A. AUSTEN and JONATHAN DERRICK. *Middlemen of the Cameroons Rivers: The Duala and Their Hinterland, c. 1600-c. 1960*.
By Jan Vansina 653

Film Reviews

- NOT FOR OURSELVES ALONE: THE STORY OF ELIZABETH CADY STANTON AND SUSAN B. ANTHONY. Directed by Ken Burns.
By Gayle V. Fischer 655
- SUNSHINE [*A Napfény íze*]. Directed by István Szabó.
By Catherine Portuges 656
- THE KNOT. Directed by Aleksandr Sokurov.
By Alexander Soifer 656
- BUENA VISTA SOCIAL CLUB. Directed by Wim Wenders; LÁGRIMAS NEGRAS. Directed by Sonia Herman Dolz.
By Darién J. Davis 657

Collected Essays

METHODS/THEORY

- NICK MERRIMAN, editor. *Making Early Histories in Museums*. 660

COMPARATIVE/WORLD

- JOHN COLES, ROBERT BEWLEY, and PAUL MELLARS, editors. *World Prehistory: Studies in Memory of Grahame Clark*. 660
- CHRISTOPHER KLEINHENZ and FANNIE J. LEMOINE, editors. *Fearful Hope: Approaching the New Millennium*. 660
- JACQUES WAARDENBURG, editor. *Muslim Perceptions of Other Religions: A Historical Survey*. 660
- RAYMOND GREW, editor. *Food in Global History*. 661

ASIA

- JAMES R. LILLEY and DAVID SHAMBAUGH, editors. *China's Military Faces the Future*. 661
- ROBERT I. ROTBERG, editor. *Creating Peace in Sri Lanka: Civil War and Reconciliation*. 661

CANADA AND THE UNITED STATES

- KATHRYN MCPHERSON, CECILIA MORGAN, and NANCY M. FORESTELL, editors. *Gendered Pasts: Historical Essays in Femininity and Masculinity in Canada*. 661
- FREDERICK E. HOXIE, RONALD HOFFMAN, and PETER J. ALBERT, editors. *Native Americans and the Early Republic*. 662
- JAN ELLEN LEWIS and PETER S. ONUF, editors. *Sally Hemings & Thomas Jefferson: History, Memory, and Civic Culture*. 662
- RICHARD WHITE and JOHN M. FINDLAY, editors. *Power and Place in the North American West*. 662

- KIMBERLY SPRINGER, editor. *Still Lifting, Still Climbing: Contemporary African American Women's Activism*. 662

EUROPE: ANCIENT AND MEDIEVAL

- MARTHA CARLIN and JOEL T. ROSENTHAL, editors. *Food and Eating in Medieval Europe*. 662

EUROPE: EARLY MODERN AND MODERN

- HARTMUT LEHMANN and ANNE-CHARLOTT TREPP, editors. *Im Zeichen der Krise: Religiosität im Europa des 17. Jahrhunderts*. 663
- KIRSTY CARPENTER and PHILIP MANSEL, editors. *The French Émigrés in Europe and the Struggle against Revolution, 1789–1814*. 663
- JOOST AUGUSTEIJN, editor. *Ireland in the 1930s: New Perspectives*. 663
- ANTHONY F. HEATH, RICHARD BREEN, and CHRISTOPHER T. WHELAN, editors. *Ireland North and South: Perspectives from Social Science*. 663
- GERD R. UEBERSCH, editor. *Der Nationalsozialismus vor Gericht: Die alliierten Prozesse gegen Kriegsverbrecher und Soldaten 1943–1952*. 664
- PEKKA ISAKSSON and JOUKO JOKISALO, editors. *Eriarvoisuus, valistuksen lupaus ja rasismi*. 664
- A. KEMP-WELCH, editor and translator. *Stalinism in Poland, 1944–1956: Selected Papers from the Fifth World Congress of Central and East European Studies, Warsaw, 1995*. 664
- KEVIN MCDERMOTT and JOHN MORISON, editors. *Politics and Society under the Bolsheviks: Selected Papers from the Fifth World Congress of Central and East European Studies, Warsaw, 1995*. 664

Documents and Bibliographies	665	Index	681
Other Books Received	668	Index of Advertisers	52(a)
Communications	676		

Topical Table of Contents

Administration	560, 595, 601, 620, 630, 645, 647, 652	Business/Finance	531, 550, 558, 621, 630, 634
Agriculture	530, 532, 565, 627	Childhood/Youth	534, 616
Biography	524, 539, 541, 547, 551, 552, 574, 582, 586, 592, 595, 600, 601, 607, 610, 611, 637, 642	Civil Wars	549, 618

- Class
 - 527, 539, 545, 570, 583, 608, 609, 612, 614, 634, 650
- Colonial/Postcolonial
 - 513, 527, 529, 530, 533, 534, 537, 583, 585, 586, 613, 615, 653
- Comparative
 - 519, 604
- Constitutional
 - 628
- Crime and Violence
 - 539, 586, 591, 595, 625, 630
- Cultural
 - 513, 515, 518, 523, 529, 532, 537, 541, 553, 566, 573, 577, 582, 586, 588, 593, 594, 595, 598, 603, 604, 608, 609, 611, 612, 614, 619, 622, 623, 625, 633, 634, 639, 643, 644, 646, 648, 650
- Demography
 - 567, 629, 632
- Diasporas
 - 555, 596
- Economic
 - 526, 529, 530, 531, 559, 562, 563, 578, 580, 581, 584, 591, 621, 627, 629, 635, 647, 648, 649
- Education/Students
 - 527, 551, 585, 611, 613, 616
- Empire
 - 521, 615, 640, 643, 644, 651
- Environment
 - 612, 649
- Ethnicity
 - 544, 556, 568, 569, 579, 593, 653
- Family
 - 538, 541, 567
- Film
 - 515
- Folklore
 - 598
- Foreign Relations/Diplomatic
 - 513, 520, 521, 522, 542, 564, 565, 578, 581, 584, 590, 602, 606, 641, 645
- Frontiers/Borderlands
 - 516, 644
- Gender
 - 518, 523, 525, 537, 541, 547, 551, 575, 614, 617, 630
- Genocide
 - 640
- Health/Disease
 - 653
- Historiography
 - 510, 511, 515, 516, 604
- Ideology
 - 519, 545, 558, 559, 624, 639
- Immigration/Migration
 - 533, 534, 555, 568, 579, 632
- Indigenous Peoples
 - 533, 542, 543, 544, 567, 586, 590, 591, 592
- Institutions
 - 566, 606, 620, 652
- Intellectual
 - 518, 523, 552, 553, 559, 566, 592, 594, 597, 600, 603, 619, 637, 639
- Journalism
 - 623
- Labor
 - 525, 550, 558, 560, 565, 573, 583
- Language/Linguistics
 - 586, 616, 633
- Legal/Legislative
 - 519, 546, 547, 568, 572, 597, 599, 633, 652
- Literature
 - 523, 588
- Local/Regional
 - 516, 529, 531, 536, 538, 539, 544, 545, 556, 557, 558, 562, 591, 618, 626, 628, 629, 632, 635, 653
- Medicine
 - 509, 510, 518, 533, 552, 573, 603, 609, 642, 653
- Memory
 - 511, 527, 577
- Methods
 - 509, 510, 511, 513
- Military
 - 521, 522, 548, 549, 578, 589, 596, 604, 606, 607, 620, 644, 645
- National Histories
 - 516, 590, 601, 607, 640, 643, 644, 651
- Nationalism
 - 626, 648, 650
- Oral History
 - 527
- Peasants
 - 627
- Philanthropy
 - 550, 616
- Philosophy
 - 600, 619, 639
- Political
 - 512, 519, 520, 521, 522, 523, 524, 525, 532, 533, 539, 542, 545, 551, 554, 556, 557, 562, 563, 564, 565, 568, 570, 572, 573, 578, 581, 584, 588, 589, 591, 595, 601, 602, 606, 607, 609, 610, 612, 615, 617, 618, 623, 624, 628, 630, 633, 635, 636, 638, 639, 640, 642, 643, 645, 646, 648, 649, 650, 651

Psychology/Psychiatry 518	596, 597, 608, 612, 614, 620, 625, 626, 627, 629, 630, 634, 635, 636, 644, 646, 653
Public History 512, 536	Social Policy 533, 547, 551, 570
Race/Racism 518, 546, 547, 548, 554, 555, 569, 570, 572, 573, 583	Technology/Industry 549, 558, 560, 562, 573, 630, 638, 647
Radicalism 555, 566, 574, 639	Theory 646
Religion 518, 527, 541, 543, 548, 551, 585, 589, 592, 597, 598, 599, 600, 603, 611, 612, 617, 628, 636, 639, 652	Trade 580, 653
Revolution/Social Movements 520, 546, 554, 574, 586, 589, 622, 626, 651	Transportation 580
Rural 586, 621, 636	Urban 512, 536, 556, 557, 562, 579, 625, 632
Science 509, 510, 552, 553, 562, 612, 637, 638, 649	Wars 526, 527, 542, 547, 548, 560, 564, 577, 581, 596, 602, 604, 607, 638, 640, 644
Sexuality 575	Women 509, 510, 524, 525, 527, 538, 547, 550, 551, 563, 574, 575, 612
Slavery 546, 547, 582, 591	World 520, 523, 526, 580, 584, 604, 642
Social History 511, 515, 524, 526, 532, 534, 536, 537, 539, 541, 549, 562, 563, 567, 579, 582, 585, 588, 591, 595,	

In This Issue

This issue contains three articles, an *AHR Forum*, and a tripartite review essay. The articles analyze knife fighting in Greece, famine and modernization in the Soviet Union, and historical consciousness in Mexico. The *Forum* discusses the ways in which understanding the history of slavery in the United States requires boundary-crossing analyses. And the review essay contains three different assessments of Alfred W. Crosby's recent study of quantification in Western society. In addition, the issue contains our usual array of book and film reviews.

Articles

Thomas W. Gallant examines the complex relationship between honor, masculinity, and violence. Using nineteenth-century records from the British colonial courts on the Ionian Islands of Greece, he advances three central propositions: Greek plebian men were exceptionally prone to violence; their violent confrontations were rooted in an ethic of honor; the most common form of interpersonal male violence was the knife fight; and knife fights were as ritualized and rule-bound as the better known aristocratic duel, and thus should be considered a form of lower-class dueling. Gallant contends that the court system imposed by the British on the islands plays a crucial role in the transformation of men's contests over honor and status. He also explains why honor has remained a critical component of the identity of Greek men even though violence has ceased to be an important element in the cultural construction of masculinity. Gallant's persuasive argument that Greek men retained honor as a cornerstone of masculinity but disassociated it from violence makes a significant contribution to our understanding of the reduction of interpersonal violence so ubiquitous in the historical record.

David C. Engerman analyzes the United States press coverage of the 1932–1933 famine in the Soviet Union. He focuses on the controversies that emerged over the press analysis of the famine to challenge the conventional story of ideologically driven journalism. Engerman explains that in most accounts journalists Walter Duranty and Louis Fischer are blamed for covering up the famine, while reporters William Henry Chamberlin and Eugene Lyons are lauded for revealing the cover-up. However, Engerman argues that the stories the four wrote shared a number of commonalities that reveal their basic agreement about the famine and

the Soviet state: stereotypes of Russian “national character,” a desire for economic progress even at high costs, and a tendency to understand Russia as an “Asiatic” society. These shared beliefs highlight the emphasis each journalist placed on the relationship between the ends and means of economic growth. Engerman also notes similarities between the American press coverage of the Soviet famine and the foreign reporting on the Chinese famine that accompanied the 1958–1960 “Great Leap Forward.” He contends that shared concerns about the ends and means of economic growth were evident not only in these instances of reporting but also in discussions of theories of modernization and development. And he asserts that these concerns have reappeared recently in debates about “Asian values” and the relationship of cultural particularism, political democratization, and economic change. Engerman’s analysis compels a new understanding of the role of economic beliefs in the analysis of events in other nations.

Thomas Benjamin assesses the recent Mayan revival in southeastern Mexico and its connection to the 1994 rebellion of peasant Mayan Indians as a way of exploring the issue of indigenous conceptions of history. He explains that the revival refers to the revitalization of indigenous culture and development of a collective Indian identity that began in the 1970s. The story of the revival focuses on the rise and fall of a monument to a conquistador, which was destroyed by indigenous protest marchers on Columbus Day 1992. The destruction of the statute, Benjamin argues, revealed a newfound understanding of history that was expressed in the symbolic act of rebellion. It was as well a precursor to the violent rebellion that erupted fourteen months later. He also contends that the event, along with the appearance of the first printed indigenous historiography, demonstrated the Mayas’ new awareness of their history, which has had a profound influence on Mayan cultural identity and political assertiveness. As a result, he concludes, the Maya are becoming their own historians and demonstrating that they are not a “people without history,” but instead indigenous Mexicans who possess the ability and right of self-representation and self-determination. Benjamin’s article forces us to examine differing notions of history and the crucial relationship between historical narrative and history as lived experience.

AHR Forum

David Brion Davis begins the *Forum* “Crossing Slavery’s Boundaries” by arguing that historians and the popular media have long parochialized slavery in the United States by viewing it as an institution of the nineteenth-century American South. Even though he acknowledges that comparative histories have helped broaden our view, Davis maintains that there is also much to be said for grasping the “Big Picture”: the interrelationships that constitute an Atlantic Slave System as well as the place of such racial slavery in the evolution of the Western and modern worlds. Drawing on a course and forthcoming book that try to present this “Big Picture,” he argues that the history of the entire New World has been dominated by the theme of slavery and freedom, including struggles over emancipation. And, despite

the increasing pressures for temporal and geographic specialization, Davis insists the resources are now available to make the theme of slavery and freedom a way of teaching world history. After offering examples of the kind of topics such a course could explore, Davis concludes by noting the pervasiveness of forced labor in the twentieth century, the relative lack of mobilized protest, and the possibility that better knowledge of the evils and triumphs of history will provide more future sensitivity to human exploitation. Comments by **Peter Kolchin**, **Rebecca J. Scott**, and **Stanley L. Engerman** complete the *Forum*. Their compelling assessments of Davis's proposal raise questions about the meaning and implications of taking a "Big Picture" approach to the history of slavery. These include issues of the definitions of unfree labor, the nature of comparative and transnational analysis, and the very meaning of broadening historical inquiries. Together, the essay and comments address both the enduring problem of understanding slavery as well as the contemporary one of teaching across traditional periods and fields.

Review Essays

The three review essays use an assessment of Alfred W. Crosby's recent book, *The Measure of Reality: Quantification and Western Society, 1250–1600*, to explore the issue of whether a distinctive quantitative perception of reality led to the imperialistic power of the West in the modern world. They each assess Crosby's assertion that the impulse to measure and count explains the rise of Western global domination, a question of recurring importance. And they do so from different analytical perspectives. **Roger Hart**, a historian of Chinese mathematics, questions whether Crosby has demonstrated a significant shift in quantification in the West or the connection between quantification and specific developments in fields like weaponry and navigation. More broadly, he argues that *The Measure of Reality* typifies a genre of world history in which the imagined communities "the West," "China," and "Islam" became the central protagonists in praise-and-blame histories of civilizations that narrated the trials, tribulations, and eventual triumph of the West. **Margaret C. Jacob**, a historian of science in early modern Europe, emphasizes the importance of the "big" questions about Western hegemony that Crosby asks. She concludes, though, that his answers are limited by their use of conventional forms of intellectual history that ignore critical issues of social and political context such as the religious elements of Western science and the commercial implications of state building. **Jack A. Goldstone**, a historical sociologist who studies revolutions and social moments, concludes the reviews with comparative analysis that raises questions about the uniqueness of Western quantitative ideas and practices. He lauds Crosby's contributions to our understanding of global history, but contends that the emergence of Western power must be explained by multicausal arguments unlike the monocausal emphasis on quantification in this book. Together, the reviews underscore the larger importance of the kind of questions that Crosby asks and the difficulties of crafting persuasive answers to them.



"Murderers' Row." This photograph from Percy Falcke Martin's *Greece of the Twentieth Century* (London, 1913), 339, shows the men incarcerated in the British prison on the island of Zakynthos just after the British relinquished the Ionian Islands to the Kingdom of Greece. They have all been sentenced for murder or attempted murder, mostly the result of knife fights. Those seated in the front row are awaiting execution. In a cross-section of Greek plebian culture, many are attired in characteristic peasant garb, while the man seated in the front left is in a shepherd's goatskin cloak (*capote*); standing next to him is an urban dandy.

Honor, Masculinity, and Ritual Knife Fighting in Nineteenth-Century Greece

THOMAS W. GALLANT

ON THE SWELTERING NIGHT OF JULY 26, 1830, Tonia Theodoros from the village of Agios Theodoros on the island of Kerkyra brutally slashed the face of his fellow villager Gioragachi Mokastiriotis. Theodoros then spit on his prostrate victim and left the wine shop where the incident had occurred, while five other men, including the proprietor Panos Landates, looked on. Ten days later, Constable Andreas Sallas approached Theodoros, served him with an arrest warrant, and took him into custody on the charge of assault with a deadly weapon. At his trial in police court on August 28, the various versions of Theodoros's assault on Mokastiriotis were recounted. There had been bad blood between the men for some time; no one was quite sure why. That night at the bar, both had been drinking heavily when Theodoros called Mokastiriotis a fool and a braggart. Mokastiriotis loudly replied that he would rather be a fool than "the lord of a house full of Magdalenes." Theodoros erupted from his chair, drew his pruning knife, and demanded that Mokastiriotis stand and face him like a man. None of the other men in the room intervened as the knife-fighters traded parries and thrusts. Finally, Theodoros with a flick of his wrist delivered a telling blow that cut his victim from the tip of his chin to halfway up his cheek. As the blood flowed, Mokastiriotis fell to his knees cursing his assailant. When asked by the presiding magistrate at the police magistrate's court in the town of Kerkyra why he started fighting, Theodoros sternly replied that no man would call his wife and daughters whores and get away with it. His reputation would not allow it. As a man, he would not stand for it. He was found guilty, sentenced to forty days (less time served) in the House of Corrections at Fort Abraham, fined three Ionian dollars, charged for court costs, and bound over to keep the public tranquility.¹

This vignette captures the three themes I address in this article: honor, mas-

¹ Istoriko Arheoi tis Kerkyras (Historical Archive of Kerkyra, hereafter IAK), Ektelestiko Astinomia (Executive Police Section, hereafter, EA) 870. The numbers recorded here refer to the file number under which the document or group of documents is filed in the archive. A file may consist of the records of a single case or a number of cases. See Alikí Nikiforou-Testone, *Arheio Ektelestikis Astinomias* (Kerkiras, 1991), for a guide to the archive and its numbering system. The second archive from which I gathered court cases is the Topiko Istoriko Arheio tis Kefallenias (Local Historical Archive of Kefallenia, hereafter, TIAK). The police and court records in this collection were being archived while I was conducting my research; the record numbers here are the ones provisionally assigned to each document file. I wish to thank the archive directors, Alikí Nikiforou-Testone at Kerkyra and Yioros Moschopoulos at Kefallenia, for their assistance. Additional research was also undertaken in the Colonial Office records (CO) housed in the Public Records Office (PRO) in Kew Gardens, England.

culinity, and violence. In anthropology, the literature on honor, or that cluster of attitudes and attributes often glossed as honor, is large and important. Ethnographers and anthropologists working in Greece have been early and critical contributors to this body of work, and consequently Greece is often considered paradigmatic of an "honor culture." While some works in this corpus indicate that honor was part of a masculine cultural code that often required displays of aggressiveness, few studies have actually discussed the role of violence in the ethos of honor. Indeed, for most of this century, Greece has manifested remarkably low rates of interpersonal violence.²

Historians working in other areas of Europe and the world have borrowed from the rich anthropological honor literature in order to understand better the cultural logic of male violence in the past. Prominent have been studies of the duel—a form of ritual male-on-male violence that drew its cultural meaning from an ethic of esteem or honor. Kevin McAleer in his recent study of the duel in Wilhelmine Germany, for example, explicitly connected the two literatures, stating, "I hope to challenge the conventional shibboleth of cultural anthropology that sees Mediterranean societies as far more sensitized to the *point d'honneur* than their neighbors to the north."³

However, historians have by and large related honor to the duel among members of elite classes; few have tried to connect it to plebeian or peasant violence. There seems to be a paradox: ethnographers have studied honor among peasants but did not discern notable manifestations of interpersonal violence; historians have studied interpersonal violence among elites and found it to be intimately connected to honor. One element of this paradox has been that historians have not studied honor among the lower orders, and they have been reluctant to accord the same ritual status to plebeian violence as they do to upper-class dueling, even though they been more than willing to see other forms of plebeian behavior as rooted in culturally constructed rituals.⁴ Another is that ethnographers have studied groups in societies where levels of interpersonal violence regardless of class have plunged to historic lows.⁵

This study aims to contribute to the existing scholarship by showing that plebeian

² Thomas W. Gallant, "Collective Action and Atomistic Actors: Labor Unions, Strikes, and Crime in Greece in the Post-War Era," in Dimitri Conostas and Theofanis G. Stavrou, eds., *Greece Prepares for the 21st Century* (Baltimore, 1995), 149–90; Gallant, "Crime, Violence, and Reform of the Criminal Justice System during the Era of Trikoupis," in Kleomenis Koutsoukis, ed., *Harilaos Trikoupis kai i epohe tou* (Athens, 1998), 17–34; Gallant, "Murder in a Mediterranean City: Homicide Trends in Athens, 1850–1936," *Journal of the Hellenic Diaspora* 24 (1998): 1–27.

³ Kevin McAleer, *Dueling: The Cult of Honor in Fin-de-Siècle Germany* (Princeton, N.J., 1994), 5. Robert A. Nye also endeavors to extrapolate from the anthropological literature on honor in contemporary Mediterranean societies to the study of honor and dueling among nineteenth-century French aristocrats and bourgeois. "Honor Codes in Modern France: A Historical Anthropology," *Ethnologia Europaea* 21 (1991): 5–17.

⁴ For a recent summary of this literature, see Edward Muir, *Ritual in Early Modern Europe* (New York, 1997). The list of works in this vein, especially for the early modern period, is long, and many of them are extremely well known and so do not require citation here.

⁵ In one of the few works that explicitly connects the historical and ethnographical data, Frank Henderson Stewart noted that many honor disputes among the contemporary Bedouin he studied ended in arbitration and settlement rather than in a duel, as they would have, among aristocrats in medieval or early modern Europe. Stewart, *Honor* (Chicago, 1994), 139–44. His interpretation is correct, but it leaves unanswered the larger question of why a defense of honor in the past called for violence whereas today it does not.

violence in the past was quite prevalent, that it was often rooted in an ethic of honor, that these forms were as ritualized and rule-bound as the aristocratic duel and, indeed, should be considered a form of lower-class dueling. It aims to move that literature forward by offering an explanation for why honor remained an integral component of Greek men's identity while dueling violence ceased to be an important element in the cultural construction of masculinity. It also shows that the court system imposed by the British on the islands played a crucial role in the transformation of men's contests over honor and status. By explicating how Greek men retained honor as a cornerstone of masculinity but dissociated it from violence, this article seeks to further our understanding of the reduction of interpersonal violence noted so ubiquitously in the historical record.

"IN MANY PARTS OF THIS COUNTRY the knife," according to Charles K. Tuckerman, U.S. consul to Greece during the 1860s, "is as quick as the tongue."⁶ The Ionian Islands, even though under the control of the British Colonial Office, were no exception.⁷ Accounts by contemporary observers and the abundant archival sources of the criminal justice system bear this out. William Goodison, for example, noted of the Ionians that the blade was "the means of directly prosecuting their revenge," and that all too frequently they were "given over to the license to raise the dark knife and bloody stiletto against the breast of unoffending innocence."⁸ Easy recourse to the dagger was commented on by nearly every other colonial officer or traveler to the islands during the period of the British Protectorate.⁹

An examination of police and court records during sixteen years of the Protectorate suggests that knife fighting was a relatively common occurrence.¹⁰

⁶ Charles K. Tuckerman, *The Greeks of To-Day* (London, 1872), 340.

⁷ The Ionian Islands (Kerkyra, Paxos, Lefkas, Ithaki, Kefallenia, Zakynthos, and Kythera) are a string of verdant isles lying off the western coast of Greece. They extend in a northwest to southeasterly arc, with Kerkyra situated in the north and Kythera in the south. In the days before steamships, they could give a state mastery of east-west maritime movement across the northern Mediterranean and so were a hotly sought-after commodity by any power with imperial pretensions in the region. By the terms of the Treaty of Paris, the islanders were granted sovereignty guaranteed by Britain, Russia, and Prussia in 1817. The United States of the Ionian Islands were to be placed under the protection of the British government. Nowhere, however, was it ever stated what form this protection should take. The situation of the Ionian Islands was anomalous. They were a sovereign polity, but another state controlled them. They were not a British colony, yet the Colonial Office administered them as if they were. The first Lord High Commissioner, Sir Thomas Maitland, set the tone for the Protectorate. He wrote the constitution, allowing for local participation but reserving real power for the Colonial Office. The British ruled the islands until 1864, when Queen Victoria ceded them to the Kingdom of Greece upon the accession of King George.

⁸ William A. Goodison, *Topographical and Historical Essay upon the Islands of Corfu, Leucadia, Cephalonia, and Zante* (London, 1822), 196.

⁹ Robert Montgomery Martin, *The British Colonies*, Vol. 6: *The Mediterranean* (London, 1856), 141; George William Orkney, Viscount Kirkwall, *Four Years in the Ionian Islands: Their Political and Social Conditions*, 2 vols. (London, 1864), 57–60; Charles J. Napier, *The Colonies: Treating of Their Value Generally and of the Ionian Islands in Particular* (London, 1833), 97–98; John Davy, *Notes and Observations on the Ionian Islands and Malta* (London, 1842), 126–27; PRO, Colonial Office records (CO) 136/692:8/27/1841.

¹⁰ I introduced an element of stratification in selecting the sixteen sample years in order to ensure coverage. Three to four years from each of the four decades of British rule were selected, but which years in each decade were determined randomly. In order to examine the operation of the system in its entirety, I recorded data from all levels: complaints and warrants, constables' reports, arrest

Based on my analyses, the average annual homicide rate on Kerkyra and Kefallenia was 12.4 per 100,000, and the combined rate for homicide and attempted homicide was 37.9 per 100,000.¹¹ Of those recorded homicides, approximately 20 percent were the result of knife fights. Both of these figures are substantially higher than the comparable rates from elsewhere in rural Europe, such as France, England, Spain, Germany, and Italy. Indeed, only mid-nineteenth-century rural Corsica manifested a higher rate.¹²

However, the homicide and attempted homicide rates do not provide a truly accurate picture of the prevalence of knife fighting among Ionian Island men. Most episodes of dagger fighting did not result in a loss of life, nor did the criminal justice system typically categorize or consider them as attempted homicides. Instead, for reasons that will soon become apparent, knife fights were classified as simple assault or assault with a deadly weapon. I recorded in the sixteen sample years 2,677 warrants or indictments for assault or assault with a deadly weapon. This translates into a rate of 134 per 100,000. From this group, I selected 125 cases (5 percent) for detailed investigation, a thorough analysis of the warrant, the police report, and the trial transcript if the case went to court. Of the 125 assaults, 61 were knife fights. In other words, in 48 percent of the recorded cases of assault, the offense involved dueling with knives. Assuming that this ratio characterizes the entire sample, over 8,000 duels would have taken place during the period from 1817 to 1864. The figures become even more revealing if we consider only assault with a deadly weapon. There were 37 of these, of which 33 (89 percent) were knife fights.

As Peter King has suggested, historians have largely neglected the study of assault and other similar forms of interpersonal violence, so it is difficult to situate my case in a comparative framework.¹³ Nonetheless, that Ionian Island men had a marked propensity to draw their blades and to use them frequently and regularly, and that violence was an integral element in masculine social discourse seems abundantly clear. This last is important. The distinctiveness in this case lay in the ready recourse Greek men had to the knife fight as a primary response to a challenge to their reputation. A close reading of the narratives of these clashes allows us glimpses of the inner logic and rules of these plebeian duels. Unlike the usual type of aristocratic or bourgeois duels studied by historians, there are no manuals to guide us in our search for the rules of knife fights. Instead, the rituals that men followed can only be excavated from the tales of the fights men told to each other, the police, and the courts.

When Ionian Island men during the nineteenth century felt that they had been insulted or had some sort of grievance against other men from their group, they frequently challenged one another to settle their differences by dueling with knives. Among European aristocrats, U.S. Southern elites, and other men who duelled,

registers, court proceedings from all levels of the judiciary, sentences and convictions, prison records, and correspondence inside the criminal justice system and between its functionaries and the colonial officers. I employ data from all of these levels in this examination of knife fighting.

¹¹ For the purposes of this article, I have not presented the rates over time. Instead, the changing patterns of homicide will be discussed in another article.

¹² Gallant, "Murder in a Mediterranean City," 12–14.

¹³ Peter King, "Punishing Assault: The Transformation of Attitudes in the English Courts," *Journal of Interdisciplinary History* 26 (1996): 43–75.

verbal insults or slights or symbolically charged physical gestures, like a slap in the face, inaugurated ritual combat. Greek men employed provocative insults drawn from their rich vocabulary of sexual words as their *causae belli*. Analogous to the slapping of the face, once certain words were uttered in specific contexts, usually a wine shop or tavern, there was no turning back. A man had to rise to the challenge or lose status. Once the knives were drawn, the men followed a known script. They aimed not to kill but to maim, not to slay but to scar. They sought to cut the face of their opponents.¹⁴ According to the stories told, the rules compelled onlookers to refrain from interfering. They were to observe and evaluate performances, not become actors themselves until a certain part of the play was reached or one of the participants deviated from the script. As soon as one man drew blood, the audience stepped onto the stage and pulled the opponents apart. The victor often would seal his triumph by spitting on his opponent or collecting a trophy, dipping his neckerchief in the blood of the vanquished or wiping the blood off his knife with it. But the story was not done with one man losing face. In many cases, the winner remained at the scene of the fight until the constables or the village authorities arrived and took him into custody. In others, the victor would leave the actual scene but make no attempt to flee the vicinity. It appears from their behavior that these men wanted to be caught. There was, it would seem, a third act to these dramas, and it was performed in a different site: a court of law. Some examples reveal the essential elements of the dominant script and illustrate some of the variations it could take.

The vignette with which I began portrays some of the key elements of the script. The men knew one another. Animosity existed between them before the fight. The confrontation occurred in a wine shop, and thus in a male-dominated public space where both were well known to the other patrons. The clash commenced with verbal sparring, and then the words that would shift the contest from tongues to knives were uttered: words that slurred the reputation of one man's female relations and that branded him as a cuckold—a *keratás*. Reputations were at stake, and in this culture those were high stakes indeed. Each man drew his blade. Based on the way they aimed their knives at each other's faces, it would seem that the intent was not to kill but to scar. So the parries and thrusts sought to inflict the cut that would settle the contest. Once it was delivered, the observers, who had previously stood aside and assessed the fight, intervened, presumably to stop the bout before further damage was done. The victor then validated his triumph by symbolically humiliating his opponent. In the aftermath of the contest, he made no effort to avoid capture; he wanted his day in court. The sentence he received of forty days, a \$3 fine, and promising to keep the peace was a common one.

This was the basic script. Although there were some slight variations on it, two

¹⁴ In numerous cultures at various points in time, scarring or cutting or in some way attacking the face of an opponent was seen as a way of branding or shaming her or him. See, for example, Valentin Groebner, "Losing Face, Saving Face: Noses and Honour in the Late Medieval Town," *History Workshop* 40 (1995): 1–15; Elliot J. Gorn, "'Gouge and Bite, Pull Hair and Scratch': The Social Significance of Fighting in the Southern Backcountry," *AHR* 90 (February 1985): 28; Kenneth S. Greenberg, *Honor and Slavery: Lies, Duels, Noses, Masks, Dressing as Women, Gifts, Strangers, Humanitarianism, Death, Slave Rebellions, the Proslavery Argument, Baseball, Hunting and Gambling in the Old South* (Princeton, N.J., 1996), 16; Robert Muchembled, *La violence au village: Sociabilité et comportements populaires en Artois du 15^e au 17^e siècle* (Turnhout, 1989), 167–83.

basic patterns predominated. In one version, the fighters were acquaintances, and a precedent cause of the contest was some previous grievance that existed between the two. (These were not, however, vengeance fights or vendetta killings. As we shall see shortly, slayings of those sorts manifested their own rituals.) Some additional cases portray the basic script of the plebeian duel in which an existing grievance set up a violent clash. Theodoros Kavvadis from Fiskardo on Kefallenia killed Gerasimos Salomon from nearby Assos. After a hard day of fishing, Kavvadis retired to a nearby tavern. Salomon, according to witnesses, shortly thereafter entered the emporium, walked up to Kavvadis, and accused him of having stolen some tackle from his boat.¹⁵ Kavvadis warned him to be wary of whom he called a thief. Knives were drawn, and a fight ensued. Salomon lunged at his opponent, but his blow went low. Rather than slashing his foe's face, as he stated in his testimony was his intent, he severed his carotid artery. Bloody death soon followed.

Giorgios Antippas from Antippata, also on Kefallenia, on the evening of July 25, 1840, killed his cousin Athanasios. The incident took place while the two were drinking wine in front of the house of another villager. In small villages such as Antippata, it was not uncommon for someone to obtain a license and serve wine at his home. Two days before, an unknown person had injured Giorgios's donkey with a rock. The quarrel began when he started accusing his cousin of having done the deed. As more wine was drunk, the banter between the two grew more heated. Giorgios then taunted him, saying that he was not man enough to take care of his ass. Athanasios responded by "giving him the horns," that is, making the gesture that accused the other man of being a cuckold. Enraged, Giorgios unsheathed his stiletto and demanded that his cousin fight him. In the midst of their fight, Athanasios tripped over a chair leg, and while off balance he fell forward at the same time as his cousin thrust forth his blade. The fatal blow entered his neck just below his Adam's apple.¹⁶

Ioannis Pelemedis from Kerkyra learned the hard way that it was best not to confront a man who made his living working with knives. Red-faced with anger, he stormed into a wine shop on Gavia Street and approached a table at which Evstathis Sklonias, a butcher, was seated with some friends. Standing before the group, he accused the butcher of having cheated his wife on a sale of meat. Sklonias nonchalantly replied by sniffing the air and announcing to the assembled group of men that he "smelled a horned one [a cuckold] in the room, and as a butcher, he knew the stench." Pelemedis drew his dagger and demanded a fight. The duel was brief. With his second stroke, Sklonias cleanly severed his opponent's left ear from his head. He then added insult to injury by asking Pelemedis if he "wanted this piece of meat [the ear] added to the bill as well?"¹⁷ Numerous other stories also adhered to this script. A grievance existed between two men. One confronted the other with it in a public place. They exchanged insults, almost invariably of a sexual nature. The knives came out, and the fight ensued.

In most cases, the precedent grievance involved not the world of praxis and material goods but cultural capital in the form of reputations. When Alexandros

¹⁵ TIAK 873:4/24/1835.

¹⁶ TIAK 1454:7/25/1840.

¹⁷ IAK, EA 1119:1/10/1856.

Salamis from the village of Agios Theodoros on Kerkyra killed Ioannis Bassianis in 1847, it was because the victim had insulted his sister by approaching her in public and slapping her spindle and distaff out of her hands. When the brother learned of it, he confronted Bassianis on the main road to the village. In front of some other peasants who were coming home after working in their fields, he challenged him to fight. The knives were drawn and used. Bassianis later died from the cut he received on his face. Presumably, he succumbed to an infection.¹⁸ This case is different from the norm in that it was not one of the duelists who was directly insulted. The following cases were more typical.

Ioannis Tsoudis entered the taverna of Spiridon Briotas in Argostoli and demanded that Panagis Magdalios stop calling him a cuckold. According to the police report, Briotas believed that Magdalios was the source of the gossip about him, gossip that Briotas's wife had told him. When confronted with the accusation, Tsoudis simply laughed. Beside himself with fury at being mocked, Magdalios swore to make him eat his words. After a fight that the witnesses all agreed was a good and fair one, Tsoudis needed thirteen stitches to close the wound on his face.¹⁹ On the night of April 23, 1835, Giorgos Koidan entered a billiard parlor in the town of Kerkyra and demanded satisfaction from Ioannis Deninzzando. Koidan had just learned that Deninzzando had been overheard calling his wife a magdalene in public. Having tracked down his wife's slanderer, he walked up to him and, while standing nose to nose, announced to the assembled group that "no man insults my *onoré*." He then drew his dagger. In the subsequent fight, he cut Deninzzando on the face and ears.²⁰

Theodoros Kirinaris did not like his paternal cousin Spiridon and, according to elders in their village of Kominata on Kerkyra, had not since childhood. Spiridon, it seems, was a bully. On December 18, 1854, matters came to a head. Theodoros had heard that his antagonist had been telling other men tales about his wife. According to the constable's report, when confronted Spiridon said to his cousin, "it's a shame that your hen won't stay in the yard." Stiletos were unsheathed. Neither man fought well, and the affray ended with Theodoros receiving a cut on his left arm. When the night patrolman arrived on his rounds, he took statements from the witnesses and the two fighters themselves, who had remained at the wine shop. On the constable's recommendation the next day, the police magistrate issued warrants for the arrests of both men for assault. Four days later, after hearing testimony, he acquitted both men with the admonition that they should behave like kinsmen and not fight.²¹ In these and numerous other examples, gossip in the form of sexual insults about one of the men or his wife (or another kinswoman) was the root cause of the fight.²²

¹⁸ PRO, CO 136/748.

¹⁹ TIAK 658:3/4/1835.

²⁰ IAK, EA 1317:4/23/1835.

²¹ IAK, EA 215: 12/18/1854.

²² Thomas W. Gallant, "Turning the Horns: Cultural Metaphors, Material Conditions, and the Peasant Language of Resistance in Ionian Islands (Greece) during the Nineteenth Century," *Comparative Studies in Society and History* 36 (1994): 717–19. As I showed in this article, the most direct challenge to another man's reputation was to call him a cuckold. It was an insult that cut to the quick because it called into question a man's capacity to fulfill in the ways prescribed in this society the duties of a man. Most frequently, however, men chose indirect means of making the accusation. The butcher

Another well-represented variant of the script involved men who were strangers or, if they were acquainted, who seemingly held no grudge against one another. The participants were often drunk, and almost invariably sexual taunts were exchanged that led to a fight. Ioannis Koursaris from Kerkyra and Athanasios Merkouris came to blows in the wine shop of Sathi Nikieravatos when one questioned the other's paternity: "we all know your mother, but your father God only knows [*sappiamo ch'è la vostra madre, ma suo padre il Dio sa*]." The onlookers intervened only after Koursaris had drawn first blood by slashing open Merkouris's cheek.²³ Spiros Petrolides was a well-known forty-two-year-old barber in the town of Kerkyra. On the night of July 17, 1860, he fought a duel with a twenty-three-year-old laborer from the village of Velonades. The latter had challenged his manhood and honor by calling his wife a magdalene. No one knew what started the trouble before that word that could not be borne was uttered. The consensus was only that the two men, after some heavy drinking, had taken a dislike to one another. The unfortunate barber received a vicious cut across his chin before the men were separated.²⁴ In these cases, strangers clashed in a public setting. Though unknown to each other, they came from the same social class and culture and thus performed the rituals of violence according to the same culturally inscribed script.

An examination of some other forms of masculine violence amply confirms the degree to which the knife fight was ritualized and rule-bound. It could be argued against my interpretation that men simply fought with what was literally to hand, that, until the imposition of strict weapons possession laws in the 1830s, most men in this culture carried edged weapons with them—such as *pslithia* (pruning knives), daggers, stilettos—and instinctively drew them when necessary. However, in addition to the more obvious point that the same script was followed so frequently that it is unlikely men were acting wholly without intentionality, an examination of those episodes in which a quarrel took place but did *not* end with a knife fight indicates that participants knew their roles in preexisting narratives.

On March 2, 1843, Athanasios Diavatos, a fishmonger in the central market of Kerkyra, confronted Antonios Avella, a twenty-three-year-old laborer, in a wine shop by the pier and accused him of having stolen his scale. They exchanged insults. Everything, it would appear, was in place for a knife fight, but instead of knives they fought with fists. The deviation from the script, I would suggest, related to the fact that Avella was not a native. He was a foreigner from Malta, and as such he was not rooted in the same cultural system as Diavatos and the other Kerkyrans.²⁵

In another case from Kerkyra, instead of fists a man used a clay pot to smash the skull of his antagonist. Once again, the man was an outsider.²⁶ In another episode where the dominant script would have called for knives, fists were used. But here

in the story recounted above humiliated his foe with a pun revolving around his own occupation. By horned one, he alluded both to a goat or ram, the smell of which he would know from being a butcher, and a cuckolded man. The insult was sufficiently ambiguous as to leave the offended party with an out if he wished to avoid a fight. But the consequence of doing so would have been a diminishment of his reputation as a man who could protect what was his, i.e., his honor.

²³ IAK, EA 1317:12/8/1835.

²⁴ IAK, EA 1334:7/17/1860.

²⁵ IAK, EA 679:1/1/1835.

²⁶ IAK, EA 1421:6/9/1832.

the sexual taunts were exchanged between a British soldier and a local Greek peasant.²⁷ A fight at the wine shop of Panagis Lefkaditis at the fort of Agios Giorgios on Kefallenia in 1832 had all the requisite elements for a knife fight. But instead, Sisifos Romanos put Chimachi Kantouzza in “pericolo di vita” (fear for his life), as the medical examiner put it, not with a knife but with a gun. Instead of cutting him with a blade, he pistol-whipped his foe.²⁸

In the majority of those episodes in which all of the elements should have resulted in a knife fight but did not, one of two differences was evident: either one of the participants was a foreigner—a *xenos*—or one was not a social equal. Knife fighting, then, was reserved for insiders, for men who were anchored in the same social milieu. The seemingly scripted nature of the fights was not a coincidence but the result of a purposeful adherence to a shared cultural practice. An examination of another type of violence prevalent among Greek men, the vendetta homicide, further highlights the distinctiveness of the knife duel.

DURING THE CUSTOMARY AFTERNOON STROLL in the village square of Pilaros on Kefallenia on September 11, 1835, Spiridon Kallihias, a farmer, approached Theodoros Maridas, an auxiliary constable, pulled a pistol from beneath his belt and fatally shot him in the head. Many in the hushed crowd were not surprised at the killing. A few people even nodded their heads in grim-faced satisfaction. Some of the dead policeman’s kinsmen ran to get their guns; others hastened to the church to ring the mourning bell. His kinswomen gathered around the body, singing ritual laments, and began the funeral rituals. Within hours, a posse of ten armed constables arrived from the district capital. Questioning of the forty witnesses elicited only stony silence: no one had seen anything. Even the deceased’s kinsmen professed no knowledge of the assailant’s identity. After seventeen days of detention during which no one could leave the village, the killer’s identity was anonymously revealed. He was captured by the posse three days later at his cousin’s house in a nearby village. A week after that, Kallihias stood at the bar in the Hall of Justice in the island’s capital. His lawyer argued he was merely defending the reputation/honor (*timi*) of his family by killing a man who had shamed them. Earlier that year, Maridas had arrested the younger brother of Kallihias for violating the British quarantine laws, leading to the youth being sentenced to life in prison. Justifiable homicide based on the notion of blood revenge was thus Kallihias’s plea, and his fellow villagers supported it. In order to prevent a “perversion of justice” and to restore the “public tranquility,” however, the tribunal rejected this line of reasoning and found him guilty of murder. Kallihias was duly executed, his corpse dipped in tar, and hung in the city square for ten days.²⁹

For sheer audacity, few men could match Bernardo Leftachi. He was the coxswain of the Sanita boat on the island of Zakynthos. While accompanying his patron, the wealthy and powerful Dr. Dimitrios Lombardos, on a visit to one of the villages attached to the estate of the former Regent of Zakynthos, Leftachi learned

²⁷ IAK, EA 1610:7/27/1835.

²⁸ TIAK 217:7/27/1832.

²⁹ TIAK 847:9/11/1835.

that his younger brother had just been killed in a fight with a man named Glassi. Having ascertained that the contest had been unfair, the coxswain returned to the island's capital and went to the main police station. There he confronted Glassi's brother, who was a constable, and in front of the desk sergeant and two other officers, he shot Glassi in the head and then stabbed him through the heart. Before the shocked constables could act, Leftachi bolted out the door. In spite of an island-wide manhunt, the avenging brother was never captured. It was widely rumored that his patron had given him refuge and helped engineer his escape from the island.³⁰

The son of a noble house on Kefallenia was seated alone in the upper dining room of his family's country estate. Two rifle shots broke the summer evening's silence. The youthful count was instantly killed as both bullets hit home. A full staff of servants was on duty and numerous peasants were working in the vicinity of the house, yet no one admitted having seen the assassins. During the course of their investigations, the police learned that the young aristocrat recently "had paid his addresses to, and then abandoned" a woman from one of the villages under his family's control. On account of the widespread belief in blood revenge, the police automatically assumed that her brothers were the ones who went to the house that night, climbed the arbor outside the dining room balcony, and killed the man. Though "universally given credit" by the authorities for having "really done the dark deed," the brothers were never charged because "none of the peasantry could be induced to come forward to present evidence which many of them, it is believed, were able to supply."³¹

These three cases typify vendetta killings on the Ionian Islands and demonstrate some of the key differences between this type of violence and knife dueling.³² Both were ritualized forms of violence but with different rules of engagement. Honor was the key to both. But the intent of a vendetta was to kill, not, as in the duel, to maim or scar. The weapon of choice was usually a firearm rather than a knife. In addition, stealth or ambush was acceptable. There was no sense that the fight had to be a fair one. On the contrary, none of the victims discussed above had a chance to respond to his assailant. Furthermore, the cause of the conflict stemmed from actions, not words. Insults alone did not initiate a vendetta. The provocative act had to be seen as a violation of communal norms. Note the very different responses of Leftachi and the kinsmen of Theodoros Maridas. Leftachi believed that his brother had died in an unfair fight, and so the act called out for blood vengeance; Maridas's family accepted his fate as just and so felt no need to cleanse their honor in Kallihias's blood. In a vendetta, the victim did not have to be the one who committed the act; any of his kinsmen was fair game. And vendetta killers vigorously sought to elude apprehension and trial. Unlike duelists, who welcomed their capture, the men discussed above evaded apprehension—even when they had committed their crime

³⁰ *Dispatches from Her Majesty's Consuls in Corfu, Zante, and Cephalonia* (London, 1867), Consul Wodehouse to Consul-General Saunders, 2/13/1865, enclosure No. 13, p. 12.

³¹ Kirkwall, *Four Years in the Ionian Islands*, 70–71.

³² Vendetta violence in Greece resembles the practice as recorded elsewhere in the Mediterranean. See, for example, Stephen Wilson, *Feuding, Conflict and Banditry in Nineteenth-Century Corsica* (Cambridge, 1988); and Christopher Boehm, *Blood Revenge: The Enactment and Management of Conflict in Montenegro and Other Tribal Societies*, 2d edn. (Philadelphia, 1987).

in a police station. As many scholars have noted, vendetta killers often could never return to their homes and so took to the hills to become brigands.³³ Finally, the loquaciousness of the witnesses to knife fights contrasts with the stony silence maintained by those who saw vendetta killings. The Greek code of *omertá* ("silence") pertained to certain types of violence but not others. This last point especially requires attention. Men wanted to tell tales about knife fights, and they seemed to relish the opportunity to recount them in court.

WHERE KNIFE FIGHTING WAS INVOLVED, Ionian Island men incorporated the British colonial criminal justice system into their masculine discourse of honor. Cases of assault or assault with a deadly weapon could be inaugurated by the victim as a private prosecutor or by the Advocate Fiscale on behalf of the state. Acting as the presiding magistrate in the police court, the Advocate Fiscale would hear testimony from the participants and the constables and other officials. His role in these cases was to determine if there was a case to be answered. In only 29 of the 125 cases I examined did he determine that the case should not go forward to the Correctional Court, the lowest rung of the criminal justice system that had a public trial. In the cases that he did not set forward, the magistrate determined that either no party was legally at fault or that the wounds sustained were negligible. In some cases, he ordered that the victim be awarded a sum of money. The vast majority of cases, however, went before the Correctional Court, where a panel of three Greek jurists would hear the case. All parties attended, and witnesses gave evidence.

In their testimonies, Ionian Island men talked about the fight: what it was over, what was said. But they also rendered an evaluation of the contest, and the judges paid heed to their assessments. Men talked about how well the fight went, whether or not the two were equally adept. Was one man drunker than the other? Did one aim to kill rather than to maim? The judges seem to have taken these factors into account. If the fight was fair and the provocation unbearable, they occasionally imposed a non-custodial sentence such as a monetary fine or the payment of a surety to keep the public peace, or a light custodial sentence of from twenty to forty days. As we shall see shortly, punishments such as these became much less common over time. Magistrates sought to learn whether or not there had been bad blood between the fighters. If there had, they usually imposed a stiffer sentence. They inquired about the course of the fight. If one man had taken advantage of the other and not played by the rules, then a term in the House of Correction was in store for the guilty. If one of the fighters had died as a result of wounds received in the fight, the judges focused on the question of intent: did the killer intend to inflict a mortal blow? The fate of the defendant hinged on how the witnesses responded to this crucial question. Intent distinguished accidental death from homicide and assault from attempted homicide. Initially at least, jail time awaited only those convicted of the latter two offenses. As time went on, however, regardless of how men answered

³³ John S. Koliopoulos, *Brigands with a Cause: Brigandage and Irredentism in Modern Greece, 1821-1912* (Oxford, 1987), 251-53; Thomas W. Gallant, "Greek Bandit Gangs: Lone Wolves or a Family Affair?" *Journal of Modern Greek Studies* 6 (1988): 269-90; J. K. Campbell, "The Greek Hero," in J. G. Peristiany and Julian Pitt-Rivers, eds., *Honor and Grace in Anthropology* (New York, 1992), 137.

these questions or if intent was or was not manifested, it became ever more probable that incarceration would result for anyone caught knife fighting.

The courtroom provided the site for the third and final act of the drama of the knife fight. It was there that the final verdict was rendered on the duel and the duelists, but witnesses, not judges, rendered it. Courts, then, were coopted, and legal ritual was subverted to serve the needs of the contestants. Reputation, not truth, was on trial, and disputes that started with words that cut ended with words as well. The trial provided the loser with an opportunity to try and salvage his reputation. He could claim that he was drunk and taken advantage of, or that his opponent had tricked him, or that the fight had in some other way been unfair. The winner got a public forum in which to heap scorn on his opponent. In the spinning out of his tale of triumph, he redoubled his assault on the public reputation of the loser. As the man in my opening vignette vowed, no man would insult his reputation and get away with it. The refrain heard most often from the victor was that he made the other man "eat his words."

Just as important, I suggest, were the stories by the witnesses. Even more than the participants, they framed the discourse for public assessment. In their narratives, men commented on every aspect of the fight: who fought well, who did not, who instigated, who got what he deserved. The discursive power of the duel to damage or salvage reputations stemmed from the comments of the witnesses, which acted to formalize the narrative. While the knife fights were usually high drama—after all, men could and did die in them—the witnesses' assessment could reduce them to farce as well. Then the reputations of both the scarred man and his unblemished foe were damaged. In the case of Andreas Magiras and Nikolaos Bamboulis in 1835, the onlookers heaped scorn on both because they were so drunk that they did not fight well. As one man told the court, "this was not serious, they fought like fools."³⁴

The narratives told in court also tell us what the fights were really about: honor. Repeatedly in their stories, men talked of reputation (*fama*, *diko mou*), honor (*timi*, *onoré*), insults and vulgar verbal assaults (*villania insolenza*). As Charles K. Tuckerman observed, the single most prevalent cause of interpersonal violence among Greeks was honor: "a wound of honor or a family insult," he remarked, "burns and did so till soothed by the blood of the insulter."³⁵ Nineteenth-century Ionian Island men were as sensitive to their reputations and status as their latter-day kindred whose preoccupations with honor have been so well documented by ethnographers.

Elsewhere, I have argued that certain words had extremely evocative power in this society because they tapped into a set of core metaphors through which men made sense of their world.³⁶ The combat with knives was part of the same discourse in which men fought with words. Men walked a very fine line in their struggles for honor and status. The banter and verbal sparring described by Michael Herzfeld and other ethnographers in contemporary Greece was a part of men's discourse in

³⁴ TIAK 724:7/14/1835.

³⁵ Tuckerman, *Greeks of To-Day*, 240.

³⁶ Gallant, "Turning the Horns," 704.

nineteenth-century Greece as well.³⁷ But Ionian men were fully cognizant that there were certain words, certain phrases that once uttered could lead to violence. These words challenged a man's honor and reputation. Greek men literally put their faces on the line in order to save face figuratively. If we accept that Greek knife fighting was a plebeian form of dueling, in what ways was it similar to dueling among the higher orders of other societies as studied by historians?

In a number of respects, Greek knife fights were remarkably similar to duels conducted among aristocrats in the U.S. South, British soldiers, French bourgeois, Italian liberals, and German students.³⁸ This is not, of course, to imply that dueling everywhere and at all times was identical. There were variations in style and form both between cultures and over time. Some general characteristics, however, are apparent. First and foremost, the Ionian knife fight and the canonical duel were ritualized single combats fought by men in the name of honor. Witnesses were central to the ritual, but their role was by and large a passive one. Unless a rule of combat was infringed or until a certain point in the ritual was reached, they were to act as observers only. The Greek duel was fought to first blood, as was the duel with swords in France and Italy. Moreover, in Greece as elsewhere, the duel acted as a boundary marker between groups and classes. Many students of the duel have emphasized that a man restored his honor and good name simply by entering the arena of combat. Dueling was not about winning or losing but about playing the game. Thus the university students who participated in the German dueling ritual *Mensur* proudly displayed their facial scars. Indeed, according to McAleer, the tactics of the sword fight developed in such a way that exposed the participants' "faces to the jagged lacerations on which every student duelist plumed himself."³⁹ Among the Greeks, a man who refused to fight lost the respect of his peers, but how those peers regarded the loser is problematic. Based on their courtroom narratives, it appears that they considered the facial scar of a bested fighter as a mark of defeat and so they held him in lower esteem. The one who was cut did not suffer shame but rather, as the Greeks put it, *systoli*—a word whose root meaning is "diminution." In other words, losing a knife duel did not *dishonor* a man. He kept his honor, but his reputation was diminished. By and large, then, for them as for duelists in other societies, the true test of a man's mettle was his willingness to fight solo

³⁷ Michael Herzfeld, *The Poetics of Manhood: Contest and Identity in a Cretan Mountain Village* (Princeton, N.J., 1985), 126–29.

³⁸ The comparative literature on the duel is vast. I have drawn mainly on the following works, Dickson D. Bruce, *Violence and Culture in the Antebellum South* (Austin, Tex., 1979); Bertram Wyatt-Brown, *Southern Honor: Ethics and Behavior in the Old South* (New York, 1982); Greenberg, *Honor and Slavery*; Edward L. Ayers, *Vengeance and Justice: Crime and Punishment in the 19th Century American South* (New York, 1984); Robert A. Nye, *Masculinity and Male Codes of Honor in Modern France* (New York, 1993), 136; Anthony E. Simpson, "Dandelions on the Field of Honor: Duelling, The Middle Classes and the Law in Nineteenth-Century England," *Criminal Justice History* 9 (1988): 99–155; V. G. Kiernan, *The Duel in European History: Honour and the Reign of Aristocracy* (Oxford, 1988); Cecilia Morgan, "'In Search of the Phantom Misnamed Honour': Duelling in Upper Canada (Conceptions of Masculine Honour)," *Canadian Historical Review* 76 (1995): 529–63; Donna T. Andrew, "The Code of Honour and Its Critics: The Opposition to Duelling in England, 1700–1850," *Social History* 5 (1980): 409–34; Ute Frevert, *Men of Honour: A Social and Cultural History of the Duel*, Anthony Williams, trans. (New York, 1995); Frevert, "Honour and Middle-Class Cultures: The History of the Duel in England and Germany," in Jürgen Kocka and Alan Mitchell, eds., *Bourgeois Society in Nineteenth-Century Europe* (Providence, R.I., 1993), 207–40.

³⁹ McAleer, *Dueling*, 123.

combat in the name of honor. Only the failure to fight would have signified emasculation.

There were a number of differences between Greek plebeian knife fights and dueling elsewhere. Greek peasants and workers had no written *code duello*. There were no equivalents to Raimund Sebetič's *Duell-Regeln* or Adolphe Tavernier's *L'art du duel*.⁴⁰ This, however, is a rather minor distinction due to class-based differences in literacy. There were more important differences. First, there was no, or only a slight, delay between the utterance of an insult and the duel among the Greeks. Combat followed hot on the heels of the attack on a man's honor. A key part of the upper-class duel was the time delay between affront and affray. Second, Greek plebeian duelists did not seek to elude capture, since, as I suggested earlier, an appearance in the courtroom was a vital component of their ritual. This is markedly different from the practice in Germany and France. McAleer shows that even the introduction of special "courts of honor" did little to alter duelists' reluctance to engage the state in honor disputes.⁴¹ And in the case of France, "both defenders and opponents of the duel agreed that a man who took his case to the courts would be perceived as a 'coward' whose sense of 'independence' was so feeble that he was willing to entrust a decision about his personal honor to complete strangers."⁴² One final aspect distinguishing the Greek knife fight from the upper-class duel of Europe and the United States is that none of the explanations proposed to account for the end of upper-class dueling can explain why Greek plebeian men kept their honor but put away their blades.

For some historians, the differences in the rituals of lower-class combat and upper-class dueling are so substantive as to mark them as two distinct forms of violence. Dickson Bruce expresses such sentiments by stressing the time delay and the nature of the combats: "The duel, unlike the street fight, was an outgrowth of the carefully cultivated style of the Southern gentry. A fit of passion may have led to the problem but duelists quickly assumed the kind of behavior of which society approved, and instituted procedures to maintain order . . . The street fight not only admitted the passions, but brought to the fore impatience, revenge, and a rejection of order. The chapters may have been the same whether one fought for revenge or dueled for honor, but the procedures and the symbolism were very different."⁴³ Others writing about dueling in the Old South also accept the distinction drawn by Bruce between honor and dueling among elite men and their absence from the social realm of plebeian men. Echoes of this view resound in the literature on the duel in Europe as well. Robert A. Nye, for example, draws a clear distinction in the role of honor between the French duel and the lower-class knife fight.⁴⁴ Anthony Simpson in his study of the English even more explicitly distinguishes the duel as a class boundary delimiter. In his view, "it would be impertinent to equate the psyches of the working-class brawler and the aristocratic duelist."⁴⁵ For some, then, it is the seeming absence of honor that is crucial.

⁴⁰ R. Sebetič, *Duell-Regeln* (Graz, 1879); A. Tavernier, *L'art du duel* (Paris, 1885).

⁴¹ McAleer, *Dueling*, 86–98.

⁴² Nye, *Masculinity*, 176.

⁴³ Bruce, *Violence and Culture*, 73.

⁴⁴ Nye, *Masculinity*, 136.

⁴⁵ Simpson, "Dandelions on the Field of Honor," 176.

For others, it is the seemingly unscripted nature of lower-class violence. Some sociologists and criminologists, for example, have recently returned to an idea proposed by Marvin Wolfgang almost thirty years ago that much masculine working-class violence was caused by “trivial slights” over honor or reputation.⁴⁶ Martin Daly and Margo Wilson, for example, have argued that “a seemingly minor affront is not merely a ‘stimulus’ to action, isolated in time and space. It must be understood within a larger social context of reputations, face, relative social status, and enduring relationships. Men are known by their fellows as ‘the sort who can be pushed around’ or ‘the sort that won’t take any shit,’ as people whose word means action and people who are full of hot air, as guys whose girlfriends you can chat up with impunity or guys you don’t want to mess with. In most social milieux, a man’s reputation depends in part upon the maintenance of a credible threat of violence.”⁴⁷ In his study of over 500 homicides involving Australian youths, Kenneth Polk concluded that the overwhelming majority of them were confrontations of honor in which young men traded insults and then blows in defense of their status among their peers.⁴⁸ While the sociological literature revises the historical studies of the duel by suggesting that lower-class violence is also connected to honor, it still espouses the notion that it was not ritualized or formulaic. Polk specifically distinguishes the cases he studied as “confrontational homicides,” thus perpetuating the idea that plebeian violence was spontaneous, unscripted, and rage-driven.⁴⁹

Pieter Spierenburg more than any other historian has shown that knife fighting among lower-class Amsterdam men was rule-bound and ritualistic and that the causes of most conflicts were slights and insults to another man’s reputation. As he concisely put it: “Masculinity and honor were central to the knife fighters’ world.”⁵⁰ Elliot Gorn in his examination of Southern backcountry lower-class violence argued that honor was directly implicated in such fights and that they were subject to their own rituals: “A rough-and-tumble was more than a poor man’s duel, a botched version of genteel combat. Plain folk chose not to ape the dispassionate, antiseptic, gentry style but to invert it. While the gentlemen’s code of honor insisted on cool restraint, eye-gougers gloried in unvarnished brutality.”⁵¹ The evidence from

⁴⁶ Marvin E. Wolfgang, *Patterns in Criminal Homicide* (Philadelphia, 1958), 188–89.

⁴⁷ Martin Daly and Margo Wilson, *Homicide* (New York, 1988), 128.

⁴⁸ Kenneth Polk, “Masculinity, Honour, and Confrontational Homicide,” in Kathleen Daly and Lisa Maher, eds., *Criminology at the Crossroads: Feminist Readings in Crime and Justice* (New York, 1998), 188–205.

⁴⁹ Polk, “Masculinity,” 191.

⁵⁰ Pieter Spierenburg, “Knife Fighting and Popular Codes of Honor in Early Modern Amsterdam,” in Spierenburg, ed., *Men and Violence: Gender, Honor and Rituals in Modern Europe and America* (Columbus, Ohio, 1998), 107. See also his essays: “Masculinity, Violence and Honor: The Long Term,” in Spierenburg, *Men and Violence*, 1–35; and “Faces of Violence: Homicide Trends and Cultural Meanings: Amsterdam, 1431–1816,” *Journal of Social History* 27 (1994): 701–16. The major limitation with Spierenburg’s work is that it is based exclusively on homicide cases. As he points out, the intent of the knife fight was not murderous, and so the examples he examined were exceptional. This restricts his ability to illuminate the normative rules of conduct that structured knife fighting. It also means that the number of examples he had at his disposal was modest. I should note in passing that the other authors in the volume did not develop Spierenburg’s idea that plebeian men duelled over honor. Daniel Boschi in his examination of knife fighting in Rome, for example, persists in denying that lower-class combats were duels. Boschi, “Homicide and Knife Fighting in Rome during the Nineteenth and Twentieth Centuries,” in Spierenburg, *Men and Violence*, 73–94.

⁵¹ Gorn, “Gouge and Bite,” 41. Gorn’s conclusions are amply supported by Ayers’s study of violence in the South, *Vengeance and Justice*, 21.

Greece supports the argument that certain forms of plebeian violence were rooted in an ethic of honor, were ritualized, and were therefore more akin to than different from upper-class dueling.

The role of the courts, however, was the one difference between the rituals of Greek plebeian knife duels and upper-class dueling that was truly distinctive. Greeks embraced them, others eschewed them. While this in and of itself is an insufficient basis for drawing a categorical distinction, it may suggest an avenue for explaining the other major difference: the ending of the duel. Studies of the duel on the Continent are remarkably consistent in concluding that it was the carnage of World War I more than anything else that caused the duel to fall into disuse. As Nye put it, "the duel perished in the trenches of the Western Front."⁵² Italy walked a slightly different path, whereon it was the war and then the advent of fascist rule that kept Italian liberals off the field of honor.⁵³ Britain, as so often happened, marched to a different drummer. On Albion's isle, dueling had more or less come to an end by the third quarter of the nineteenth century. Contributing factors seem to have been the democratization of the duel, which led to the practice ceasing among the aristocracy, vigorous action of the state to curtail dueling in the military (where it had been most prevalent), and a shift in masculine sensibilities among middle-class men that transformed attitudes about violence.⁵⁴ Edward L. Ayers has proposed a complex explanation to account for the demise of the duel in the U.S. South. He sees the Civil War acting in much the same way that World War I did in Europe to generate a revulsion against violence, but he also suggests that evangelicalism and the spread and adoption of a bourgeois business ethos from the North to the South led to diminution of the importance of honor among the upper class.⁵⁵ As honor lost its saliency, dueling was diminished in importance.⁵⁶ The cultural arguments proposed by Ayers and Simpson for the duel follow along the lines of other scholars attempting to explain the general decline of violence in the West, which all derive from Norbert Elias's idea of the "civilizing process."⁵⁷ None of these satisfactorily explains why Greek plebeian men stopped dueling when they

⁵² Robert A. Nye, "The End of the Modern French Duel," in Spierenburg, *Men and Violence*, 82–101, quote 83; McAleer, *Dueling*, 201; Frevert, *Men of Honour*, 192; and Ute Frevert, "The Taming of the Noble Ruffian: Male Violence and Dueling in Early Modern and Modern Germany," in Spierenburg, *Men and Violence*, 37–63, quote 60.

⁵³ Steven Hughes, "Men of Steel: Dueling, Honor and Politics in Liberal Italy," in Spierenburg, *Men and Violence*, 64–81.

⁵⁴ Simpson, "Dandelions on the Field of Honor," 139–40; Peter N. Stearns, *Be a Man! Males in Modern Society* (New York, 1990), 49–50.

⁵⁵ Ayers, *Vengeance and Justice*, 271.

⁵⁶ Richard E. Nisbett and Dov Cohen assert that contemporary Southerners are more prone to violence than other Americans because of the persistence of a "culture of honor" in the South. However, neither in the national-level surveys they used nor in the social-psychological tests they conducted on University of Michigan students was any coherent definition of honor employed. In fact, they seem to conceptualize honor very differently from historians of the Old South, and so their work does not convincingly challenge the view that honor and dueling ebbed simultaneously. Nisbett and Cohen, *Culture of Honor: The Psychology of Violence in the South* (Boulder, Colo., 1996).

⁵⁷ Norbert Elias, *The Civilizing Process*, Edmund Jephcott, trans. (Oxford, 1994), 447–49; see also Jonathan Fletcher, *Violence and Civilization* (Cambridge, 1997). Eric A. Johnson and Eric H. Monkkonen, "Introduction," in Johnson and Monkkonen, eds., *The Civilization of Crime: Violence in Town and Countryside since the Middle Ages* (Urbana, Ill., 1996), 1–16. Pieter Spierenburg, "Long-Term Trends in Homicide: Theoretical Reflections and Dutch Evidence, Fifteenth to Twentieth Centuries," in Johnson and Monkkonen, *Civilization of Crime*, 63–105.

did. There was no war that rocked their world. There was no evangelical religious movement or a transformation of masculine sensibilities based on bourgeois values. We have to seek, like Ayers and Simpson, a complex and multicausal explanation for the demise of the working-class duel in Greece, and our explanation must account for why the duel ended but honor remained.

The literature on the duel strongly suggests that, when the practice ceased, honor also lost its central position in the core of masculine identity. As Peter Stearns has noted, in the new bourgeois society of the West, "honor [became] an archaic remnant."⁵⁸ But honor has been seen as a central element in contemporary Greek and Spanish rural societies since the ground-breaking ethnographies of J. K. Campbell, Julian Pitt-Rivers, and J. G. Peristiany.⁵⁹ Subsequent studies in these countries and in other regions of the Mediterranean led to the assertion that the "honor and shame complex" was a central defining element of a Mediterranean culture system.⁶⁰

There are clusters of actions and beliefs that recur cross-culturally and historically among cultures that profess to hold "honor" dear. Two such recurring patterns concern us. First, status, reputation, and esteem are assessed, bestowed, or rescinded by the court of masculine public opinion. Because of this, honor acts as a cultural boundary delimiter. Second, honor is intimately connected with aggression. Manhood, because it is on public display, is always at risk, and so a man's reputation is directly connected to his ability to be seen as able to defend openly against all challenges. Thus, in honor cultures, there develops an "agonistic ethos [that] has deep roots in the atomized social context of Mediterranean society. The grim conflicts it embodies are real and pervasive; the interpersonal aggressions it spawns are not gratuitous."⁶¹ Part of being a man in an honor society is a readiness to employ physical violence to defend one's reputation.

⁵⁸ Stearns, *Be a Man!* 50.

⁵⁹ J. K. Campbell, *Honour, Family and Patronage: A Study of Institutions and Moral Values in a Greek Mountain Community* (Oxford, 1964); Julian Alfred Pitt-Rivers, *The People of the Sierra* (1955; Chicago, 1961); and Pitt-Rivers, *The Fate of Shechem; or, The Politics of Sex: Essays in the Anthropology of the Mediterranean* (New York, 1977); J. G. Peristiany, ed., *Contributions to Mediterranean Sociology* (The Hague, 1968).

⁶⁰ The heated debate over whether or not there is such a cultural system need not concern us here. I am very aware of the dangers of reducing the varieties of contextually shaped behaviors down to a single gloss of honor. Nor do I argue that honor, as an ethical concept among Greek men, has remained constant through time. What I do contend, however, is that there is a connection between honor as ethnographers have recorded it in Greece during the recent past and the set of attitudes and behaviors that men during the nineteenth century described as honor. The literature on honor in the Mediterranean is vast. The major works consulted were J. Davis, *People of the Mediterranean: An Essay in Comparative Social Anthropology* (London, 1977); Anton Blok, "Rams and Billy-Goats: A Key to the Mediterranean Code of Honour," in Eric R. Wolf, ed., *Religion, Power and Protest in Local Communities: The Northern Shore of the Mediterranean* (Amsterdam, 1984), 51–70; David D. Gilmore, ed., *Honor and Shame and the Unity of the Mediterranean* (Washington, D.C., 1987). Those arguing against such a view include Michael Herzfeld, "Honour and Shame: Problems in the Comparative Analysis of Moral Systems," *Man* 15 (1980): 339–51; Herzfeld, "The Horns of the Mediterraneanist Dilemma," *American Ethnologist* 11 (1984): 439–54; Josep R. Llobera, "Fieldwork in Southwestern Europe: Anthropological Panacea or Epistemological Straightjacket?" *Critique of Anthropology* 6 (1986): 25–33; Victoria A. Goddard, "From Mediterranean to Europe: Honour, Kinship and Gender," in Goddard, Josep R. Llobera, and Cris Shore, eds., *The Anthropology of Europe: Identity and Boundaries in Conflict* (Providence, R.I., 1994), 57–92.

⁶¹ David D. Gilmore, *Aggression and Community: Paradoxes of Andalusian Culture* (New Haven, Conn., 1987), 43. See also Gilmore, *Manhood in the Making: Cultural Concepts of Masculinity* (New Haven, 1990); and Elvin Hatch, "Theories of Social Honor," *American Anthropologist* 91 (1989): 341–54.

The Mediterranean ethnographic record is replete with vivid descriptions of colorful displays of masculine bravado. But there is a paradox: for all the macho posturing, anthropologists record very few actual acts of violence, especially lethal violence. Nor do official court and police documents record many instances of honor homicides. In other words, there is a great deal of masculine bluster but little blood. In his study of the Sarakatsanoi, a group renowned for its supposed violent propensities, Campbell noted that "knives are pulled with great bravado [only] when it is certain that others are present to prevent their use."⁶² During the time period he studied the group, he saw many displays of aggression but few actual conflicts. Robert Paine elaborated slightly on this theme. In his view, the Sarakatsanoi construct zero-sum dramas in their contests over honor rather than zero-sum fights.⁶³ Michael Herzfeld in his work on Cretan shepherds, another group with a reputation for violence, found that men competed with one another at cards and contested status by trading sung insults, a Greek type of "playing the dozens" (a game among U.S. inner-city black youths of testing your opponent's wits with provocative remarks about his mother). He also noted that aggressive displays and boisterous announced threats of violence were common, though actual conflicts were few. As he concluded, what mattered to Glendiot men was to be seen to be good at being a man.⁶⁴ In Spanish Andalusia, Pitt-Rivers found that assailants squared off only when they were sure that the onlookers were prepared to pull them apart.⁶⁵ Finally, in his wide-ranging examination of aggression and honor, David Gilmore observed no cases of actual life-threatening violence; instead, he concluded that masculine aggression in Spain had by and large been channeled into symbolic displays of machismo.⁶⁶ Even among contemporary groups known for their sensitivity to honor in Greece and elsewhere, then, violence in defense of reputation has been largely reduced to ritualized, symbolic forms. This was definitely not the case in the past. When and why did this crucial transformation take place?

As noted earlier, Greek knife fighters used the court systems in ways unlike duelists elsewhere, and an examination of the role of the courts can provide a starting point for explaining the end of the duel. The criminal justice system offered a possible remedy to Greek men with a sullied reputation: slander. Over the course of the Protectorate, thousands of men and women filed warrants, attended magistrate's tribunals, and went to the Correctional Court to prosecute fellow islanders for slander. Women also used the courts in their contests over reputation, and they extended their networks of gossip to include the criminal justice system.⁶⁷

⁶² Campbell, *Honour, Family and Patronage*, 151.

⁶³ Robert Paine, "High-Wire Culture: Comparing Two Agonistic Systems of Self-Esteem (Marri Baluch of Pakistan and Sarakatsani of Highland Greece)," *Man* 24 (1989): 667.

⁶⁴ Herzfeld, *Poetics of Manhood*, 47.

⁶⁵ Pitt-Rivers, *Fate of Shechem*, 32; in a more recent ethnography from the Iberian Peninsula, Miguel Vale de Almeida noted that when men fight "there is always someone there to restrain the contenders just in time." Almeida, *The Hegemonic Male: Masculinity in a Portuguese Town* (Providence, R.I., 1996), 102.

⁶⁶ Gilmore, *Aggression and Community*, 11. Stanley Brandes as well demonstrated how Andalusian men channeled their aggressive confrontations with other men into rituals such as joke-telling, skits, and riddles. Brandes, *Metaphors of Masculinity: Sex and Status in Andalusian Folklore* (Philadelphia, 1980).

⁶⁷ Thomas W. Gallant, "'We're All Whores Here!': Women, Slander, and the Criminal Justice System," in *Experiencing Dominion: Culture, Identity and Resistance in Britain's Greek Empire* (unpublished ms.).

Men's knife fighting and women's use of the slander laws served two different strategies embedded in the same discourse over the honor and reputation of the household. Could taking a man to court for slander, almost invariably involving the same sexual insults that inaugurated knife fights, restore a man's damaged reputation? Some inferences drawn from an examination of over two hundred slander cases suggest possible answers to this query. In addition, they can provide an explanation that can connect the historian's world of the honor-based duel to the anthropologist's domain where masculine honor rules but without high levels of interpersonal violence.

The first aspect that stands out is that members of the aristocracy appear far more frequently in slander trials than in knife fights. In just over 10 percent of slander trials, one or both of the litigants was an aristocrat, whereas no aristocrats were prosecuted for assault or assault with a deadly weapon. Unlike in the cultures most often studied by historians, the Ionian aristocracy did not duel. There were some cases of members of the signori killing one another, but these were rare and were not the result of a ritualized duel. The nobility earlier and disproportionately took each other to court; however, I do not want to overstate this point: for the most part, they neither dueled nor litigated. The reason, I suggest, is that the aristocracy on these islands was organized into relatively tight patrilineal kin groups, each of which employed gangs of armed tough guys or *bravi*. Aristocratic factions fought one another by proxy, using their lower-class gangs. In addition, given that the ethos of the vendetta was prevalent, signori had to be very careful about instigating a confrontation because by so doing they could incite a potentially destructive feud. In certain respects, then, plebeian men had a more autonomous sense of self-identity than did their noble brethren, and as a consequence they had to defend their reputation in different ways. One very important way was the knife duel. The courts gained legitimacy as an arbiter of honor first among the aristocracy.

Plebeian men soon followed suit and flooded the court docket with their accusations of criminal slander. A statistical trend analysis of complaints and arrests for assault and assault with a deadly weapon shows a decrease over time. At the same time, the propensity of men to swear out complaints for slander rose.⁶⁸ To be sure, knife fighting persisted, but the relative balance of dueling to litigation shifted. The lawsuit, then, gradually replaced the duel. But no single element can account for why Greek men began to place less emphasis on violence and more on the courts as the means of preserving their reputation.

A number of factors contributed to this change. Cultural and ideological developments could have contributed to a "civilization" of male tendencies to violence. Since the 1830s, pro-British Ionians, who disproportionately held the levers of local political power, used the law and public policy to transform the national character of their countrymen. They took their cue from the colonial officers, who denigrated the Ionians, comparing them to the aboriginal groups like the Hottentots or Irish. Indeed, the British frequently referred to the Ionians as the "Mediterranean Irish."⁶⁹ The Greek civic leaders learned from the British in no

⁶⁸ The formula describing the trend for assaults is $Y_t = 2297 + 3.04 * t$, while the equation for slander is $Y_t = 113.3 + 1.93 * t$.

⁶⁹ Thomas W. Gallant, "European Aborigines and Mediterranean Irish: Identity, Cultural Stereotypes and Colonial Rule," in *Experiencing Dominion*.

uncertain terms that it was the propensity for violence that separated “Orientals” and “savages” from “Western men.” A key part of the civilizing mission launched by the Greek leadership thus focused on the restoration and maintenance of public order.⁷⁰

The Orthodox church was brought into the struggle against crime and violence through the practice of excommunicating criminal offenders.⁷¹ Standing before assembled communities of Greek peasants, dark-robed clerics pronounced the ultimate sanction—eternal damnation—against certain criminal offenders, and they reserved their sternest opprobrium for those who had committed acts of violence. While the policy was not a great success at either diminishing crime, especially crimes against property, or at reducing the influence of the code of *omertá*, nonetheless the constant preaching of the clergy against violence would have resonated with some men because of the close interconnection between Orthodoxy and Hellenism.⁷² Greeks drew a distinction between the church as an institution and Orthodoxy, and Greek men held ambiguous and conflicted views about the church. Because a key element in the Greeks’ conception of their identity as Hellenes was the Orthodox faith, pronouncements that challenged their status as Christians would have captured men’s attention.

Finally, and most important, after 1850 the leaders of the Ionian Greek nationalist movement changed their tactics from encouraging violent agitation against the British to emphasizing nonviolent forms of resistance.⁷³ During the 1830s and 1840s, the radical unionists fomented public unrest whenever possible. Public processions frequently turned into riots. Political rallies and elections were used as vehicles to whip up anti-British sentiments, and such events often ended in bloodshed. Nationalist fervor culminated in 1848 when violent uprisings rocked a number of the islands. In the aftermath, the nationalist leaders changed tack and opted for political disruption rather than open confrontation as the best means to drive the British off the islands. They sought to make the islands ungovernable by disrupting the political process. A new rhetoric accompanied this change. Violence now was constructed as an impediment to the nationalist struggle.⁷⁴

In sum, their political leaders told the people that violence was “non-Western” and uncivilized; their priests told them that it was immoral and unchristian; their radical nationalist leaders told them it was anti-Greek and hindered the struggle for national union. It would be surprising, then, if Greek plebeians did not begin to internalize new values that militated against violence, especially given that other forces were also at work that reinforced this message.

Laws were passed that forbade the wearing or possession of bladed weapons or implements in public. Only butchers were exempt from these regulations. Farmers

⁷⁰ Thomas W. Gallant, “Creating ‘Western Civilization’ on a Greek Island,” in *Experiencing Dominion*.

⁷¹ Thomas W. Gallant, “Peasant Ideology and Excommunication for Crime in a Colonial Context: The Ionian Islands (Greece), 1817–1864,” *Journal of Social History* 24 (1990): 1–32.

⁷² Thomas W. Gallant, “‘We Are the Christians’: Religion, Identity and Resistance,” in *Experiencing Dominion*.

⁷³ B. Knox, “British Policy and the Ionian Islands, 1847–1864: Nationalism and Imperial Administration,” *English Historical Review* 99 (1984): 503–29.

⁷⁴ Michael Pratt, *Britain’s Greek Empire: Reflections on the History of the Ionian Islands from the Fall of Byzantium* (London, 1978), 140.

TABLE
Custodial Sentences for Men Convicted of Assault or Assault with a
Deadly Weapon Involving a Knife Fight

<i>Number of Days</i>	<i>Percentage of Sentences (1823–24)</i>	<i>Percentage of Sentences (1847)</i>
0–5	45.2	0
6–10	20.0	1.7
11–15	9.8	3.4
16–20	10.9	0
21–30	6.5	3.8
31–40	5.4	12.1
41–50	0	0
51–80	2.2	41.3
81–120	0	19.0
121–200	0	1.7
>200	0	17.2

and farm laborers could only possess pruning knives with a blade length of less than two inches. Enforcement of the laws banning possession was one of the primary duties of the Greek police, and the strict application of the laws acted to take weapons out of men's hands. The courts increasingly took this offense seriously. In 1823, for example, the average sentence imposed for illegal possession of a knife was five days in the House of Correction. The same offense in 1855 would bring six to eight months at hard labor.⁷⁵ A trend analysis of arrests for illegal arms possession shows a very marked increase over time.⁷⁶ In short, the police became ever more vigilant in arresting men for possession of a knife in public, and the courts began to impose stiffer sentences. The end result was that fewer men had knives.

Perhaps most important, the courts treated knife fighting very differently over time. The pattern of sentencing imposed by the court on the guilty is depicted in the Table, which compares the sentences imposed by the Correctional Courts in 1823–1824 and 1847.

The difference is stark and remarkable. During the 1820s, nearly one-third of cases resulted in a non-custodial sentence. Instead of prison, the guilty party was fined a sum ranging from 1 to 5 Ionian dollars and bound over to maintain the “public tranquility.” Based on the courtroom testimonies, it appears that the decision to impose a custodial sentence and its length were based on the details of the fight and the nature of the wounds inflicted. Modal sentences were ten days and twenty days. By the latter part of the 1840s, however, judicial attitudes had changed dramatically. Everyone convicted received a jail term. The modal sentences in this period were sixty days (accounting for slightly over one-quarter of all cases) and two years. Indeed, almost 40 percent of those found guilty of slander in 1847 received

⁷⁵ I tabulated these figures from the database of convictions compiled from the court records in Kerkyra as described in note 9.

⁷⁶ The formula for arms possession arrests is $Y_t = 2.1 + 1.25 * t$. The arrest rate rose by a multiple of six between 1820 and 1858.

sentences of ninety days or more, whereas no one had received a jail term of more than sixty days in 1823 and 1824. Knife fighting went from being a relatively minor offense to one that brought with it a hefty sentence to the House of Correction.

From the beginning of the period I examined, the courtroom discourse was an integral part of the knife duel, constituting, as I argued earlier, the third act in these narratives. By incorporating the court into their daily dramas about honor and masculinity, plebeian men bestowed legitimacy on it. The nobility had shown that the courts could provide a vehicle for disputing reputations through the charge of criminal slander, and plebeian men used this new venue to contest honor in an ideological climate that was increasingly hostile to interpersonal violence. Thus, in an act of social mimesis, some plebeian men opted for the docket over the blade. For the reasons cited above—especially the confiscation of their knives and the stiffer sentences imposed by judges—men continued to contest reputations in the court of public opinion, but now, rather than being restricted solely to the enclaves of male sociability—the wine shops and taverns—the site for their contests was the Hall of Justice.

To be sure, the knife fight did not disappear, and the heightened stakes may even have held an attraction for men who saw greater honor in greater risks. But the tide had turned. Beginning around mid-century, more men who suffered the slings and arrows of outrageous insults took their antagonist to court rather than cut his face. By the 1880s, the assault rate on the islands had fallen from 134 to 27 per 100,000, and by the 1920s, it fell even further to 8.7.⁷⁷ The Eliasian civilizing process had occurred, and the knife duel as a means of defending one's honor became a relic of the past. Honor, however, did not.

ELIAS ARGUED THAT the culture of honor predominating in early modern Europe was prone to violence precisely because honor and violence were causally connected. Men of honor fought, and they fought over honor. For him and for the historians who have followed his lead, the duel was a perfect manifestation of this connection. He argued further that the rise of a "bureaucratic ethos" during the nineteenth century supplanted the ethic of honor and that alternative mechanisms of dispute resolution arose. Their advent severed the connection between masculinity and violence.⁷⁸ Studies of the changing ideas about masculinity in the West during the nineteenth century suggest that the civilizing of working-class men went hand-in-hand with industrialization and urbanization. In the factory, men internalized ideas about discipline and self-control; these were reinforced by the new civic institutions that developed in the burgeoning cities of Western Europe and the United States.⁷⁹ New ideas about middle-class male gentility were rooted in the

⁷⁷ Gallant, "Murder in a Mediterranean City," 11–12.

⁷⁸ Elias, *Civilizing Process*, 292–300; for an application of this model to France, see Nye, "Honor Codes," 10–11.

⁷⁹ Stearns, *Be a Man!* 48–107; Jeffrey S. Adler, "'My Mother-in-Law Is to Blame, but I'll Walk on Her Neck Yet': Homicide in Late Nineteenth-Century Chicago," *Journal of Social History* 31 (1997): 253–77; Martin J. Wiener, "The Victorian Criminalization of Men," in Spierenburg, *Men and Violence*, 149–63.

emerging white-collar professions.⁸⁰ In this new world, “respectable” men were assertive, not aggressive, and violence became unacceptable.⁸¹ In the southern United States, the way of life of the aristocracy was markedly changed in the postbellum period. What we see, then, in the areas where the honor-based duel flourished, is that its demise was accompanied by radical and profound changes in the material infrastructure of the dueling class. The cultural logic that had given honor its privileged place in defining masculinity had changed, and honor disappeared with the duel.

This study of plebeian violence in nineteenth-century Greece shows that honor was a more malleable concept than has been previously allowed. In this case, the ethic of honor persisted, but the intimate connection between it and violence changed. What remained of plebeian violence and masculinity were their symbolic manifestations, so well documented in mid-to-late twentieth-century ethnographies. The Greek men studied by Campbell, Herzfeld, and other ethnographers still contested status and reputation in a variety of ways but without the high incidence of interpersonal bloodshed that I have documented in this study of knife dueling. The marked decrease in masculine violence was not associated with a shift from an ethos of honor to something else.

There are a number of major reasons why Greece was different. First, unlike in the other cases where honor and dueling disappeared, in the Ionian Islands, the material basis of life that undergirded honor's role in defining masculinity did not change substantially until the very recent past. The islands experienced neither industrialization nor urbanization. Men were still mostly peasant farmers, shepherds, dock workers, and merchant seamen. Theirs remained a world of stiff competition for scarce resources, and thus the reasons why honor developed in the first place persisted. Second, the ethical foundation of honor remained intact. The campaign waged by politicians, priests, and nationalists was against violence, not honor. No Orthodox priests decried honor from the pulpit, as Evangelical and Utilitarian clergymen did in England.⁸² Far from denigrating honor, Ionian radicals steeped their nationalist rhetoric in the discourse of honor. In short, no agency or group on the islands challenged the notion that honor was at the core of what it was to be a Greek man. Finally, the courts and the legal system proved central in perpetuating the importance of honor in the discourse of masculine identity. One of the unintended consequences of the British Empire's endeavor to insinuate law and the courts into the heart of colonial rule on the islands was to make the courts relatively cheap and accessible.⁸³ As the courts gained legitimacy among plebeian

⁸⁰ Stearns, *Be a Man!* 108–54; E. Anthony Rotundo, *American Manhood: Transformations in Masculinity from the Revolution to the Modern Era* (New York, 1993); Michael S. Kimmel, *Manhood in America: A Cultural History* (New York, 1996); John Tosh, “New Men? The Bourgeois Cult of Home (in the 19th Century),” *History Today* 46 (1996): 9–16; and Tosh, “What Should Historians Do about Masculinity? Reflections on Nineteenth Century Britain,” *History Workshop Journal* 38 (1994): 179–202.

⁸¹ Angus McLaren, *The Trials of Masculinity: Policing Sexual Boundaries, 1870–1930* (Chicago, 1997), 111.

⁸² Wiener, “Victorian Criminalization,” 201, 204; Tosh, “What Should Historians Do,” 180–81.

⁸³ One of the Lord High Commissioners of the islands, Lord Seaton (formerly Sir John Colborne), put it best when he wrote that it was the aim of the Colonial Office's legal system “to bring justice to the peasant's own door.” PRO, CO 136/120, Seaton to Stanley, Zakynthos, October 10, 1843.

men as a venue for contesting reputations, their disputes took on less violent and more symbolic forms. However, honor remained central and prominent in slander trials. In each of the thousands of courtroom dramas over criminal calumny, Greek men stood in the dock and through their words reshaped what it meant to “be a man” in their society. The connective tissue between honor, masculinity, and violence was severed. Honor remained at the heart of Greek masculine identity, but violence did not.

The transition from combat with knives to duels with words took place in the nineteenth-century courtroom. When the British relinquished the islands in 1864, the Kingdom of Greece took them over and imposed its own legal system. The courts ceased to be as open and accessible, even after the major legal reforms of the 1880s and 1890s.⁸⁴ With access to the rituals and formal mechanisms of the courts removed, masculine contests over honor and reputation returned to the more informal settings of the tavern or the wine shop, where they would be studied by anthropologists in the 1950s and 1960s, but with a difference. Violence and the knife duel had been disconnected from cultural concepts of honor and masculinity. In more recent times, then, men dueled over honor with words, not with stilettos. They bested one another with cards, not blades. Losing one’s face increasingly became a figure of speech rather than a fact of life in the rough and tumble world of Ionian Island men.

⁸⁴ Gallant, “Crime, Violence, and Reform of the Criminal Justice System during the Era of Trikoupi,” 13–16.

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Modernization from the Other Shore: American Observers and the Costs of Soviet Economic Development

DAVID C. ENGERMAN

THE SPECTER OF COMMUNISM haunted not just nineteenth-century Europe but most of the world in the twentieth century. The specter of Soviet Communism haunting this century, however, was as much a blueprint for rapid industrialization as an ideology of proletarian revolution, national liberation, or totalitarian control. At the same time, the Soviet specter often bore little resemblance to actually existing circumstances in the Soviet Union itself. In spite of the tremendous costs, including a catastrophic famine in 1932–1933, domestic and foreign commentators widely praised Soviet efforts at economic modernization, especially in the early years of the Five-Year Plans (1928–1937).¹ What American diplomat George F. Kennan termed the “romance of economic development” captivated a wide range of foreign observers of all political persuasions.² These interwar observers valued the fruits of rapid industrialization above its costs—even when these costs included not only repression and privation but also starvation. Many Western observers, ranging from fellow-travelers to anticommunists, summed up their balance sheets on the Soviet Five-Year Plans with the frequently repeated canard that the USSR was “starving itself great”—a phrase that appeared well before the devastating 1932–1933 famine. In Europe’s colonies, political leaders as well as intellectuals enthusiastically endorsed the Soviet goal of rapid industrialization as a shortcut to economic

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¹ On “bourgeois” professionals’ support for the early Five-Year Plans in the USSR, see David R. Shearer, *Industry, State and Society in Stalin’s Russia, 1928–1934* (Ithaca, N.Y., 1996). On Americans’ attractions to the Soviet Union in this era, see Peter G. Filene, *Americans and the Soviet Experiment, 1917–1933* (Cambridge, Mass., 1967), esp. chaps. 7–9; Marcello Flores, “The American Attitude toward the First Soviet Five-Year Plan,” *Storia nordamericana* 1 (1984): 72–98; and Lewis S. Feuer, “American Travelers to the Soviet Union, 1917–32: The Formation of a Component of New Deal Ideology,” *American Quarterly* 14 (Summer 1962): 119–49. More cosmopolitan perspectives are offered in David Cate, *The Fellow-Travelers: A Postscript to the Enlightenment* (New York, 1973); and Joog Bachmann, *Zwischen Paris und Moskau: Deutsche bürgerliche Linksintellektuelle und die stalinistische Sowjetunion, 1933–1939* (Mannheim, 1995).

² “Memorandum for the Minister,” August 19, 1932, enclosed in Robert Skinner to Secretary of State, August 19, 1932, 861.5017 Living Conditions/510, State Department Decimal File, Record Group 59, U.S. National Archives. (Decimal file documents hereafter referred to as SDDF.)

modernity. Indian leader Jawaharlal Nehru, for instance, drew inspiration for India's planning efforts from the Soviet Union. As early as 1936, he recognized the costs of Soviet industrialization but stressed instead its benefits: despite its "defects, mistakes, and ruthlessness," Nehru wrote, Soviet industrialization is "stumbling occasionally but ever marching forward."³

Nehru's later encomium that the Soviet Union beckoned as a "bright and heartening phenomenon in a dark and dismal world" suggests that the enthusiasm for Soviet economic planning was a result of the depression of the 1930s. The economic crisis that shook the global economy heightened the perception of differences between the Western and Soviet economies—and undoubtedly brought more kind words for the latter. But Western interest in Soviet economic policy predated the 1929 Wall Street crash and the widespread acknowledgement (two years later) of a serious economic downturn.⁴

This essay argues that a wide range of interwar Russia experts in the United States, irrespective of their attitude toward the Soviet Union (or toward communism), shared a fervent belief in rapid economic development. These experts, furthermore, deployed longstanding national-character stereotypes in support of their beliefs: claims of Russians' "innate" passivity and conservatism pervaded Western reports on Soviet economic events. Later, faith in economic development combined with national-character stereotypes to contribute to Western misunderstandings of China's Great Leap Forward (1958–1960), which bear eerie similarities to earlier views of the Soviet Five-Year Plans. And in less obvious but nevertheless significant ways, the calculus of ends and means embedded in the sentiment that Russia was "starving itself great" undergirded Western theories of modernization and development in the 1950s and 1960s.

The famine of 1932–1933 looms large in any calculations of the costs of the rapid industrialization in the USSR. Leaving perhaps as many as 8 million dead, the famine devastated the principal breadbaskets of the Soviet Union: Ukraine, the Volga valley, the North Caucasus region, and Kazakhstan.⁵ The famine's legacy

³ Jawaharlal Nehru, introduction to M. R. Masani, *Soviet Sidelights* (1936), rpt. in *Selected Works of Jawaharlal Nehru* (New Delhi, 1972), 7: 128–29.

⁴ Jawaharlal Nehru, *Toward Freedom: The Autobiography of J. Nehru* (New York, 1941), 230–31. On recognition—in 1931—of the seriousness of the economic downturn, see Christina D. Romer, "The Great Crash and the Onset of the Great Depression," *Quarterly Journal of Economics* 105 (August 1990): 597–624. One Moscow-based journalist noted widespread business confidence while on an American lecture tour in 1931; Eugene Lyons, *Assignment in Utopia* (New York, 1937), 399. On the Depression's impacts on global agriculture, see Charles P. Kindleberger, *The World in Depression, 1929–1939* (Berkeley, Calif., 1973), chap. 4; and Vladimir P. Timoshenko, *World Agriculture and the Depression*, *Michigan Business Studies* 5, no. 5 (1933).

On American attractions to Soviet economic organization in this era, see Filene, *Americans and the Soviet Experiment*, chaps. 3–9; and Deborah Fitzgerald, "Blinded by Technology: American Agriculture in the Soviet Union, 1928–1932," *Agricultural History* 70 (Summer 1996): 459–87.

⁵ On the demographic impact of the famine, see E. A. Osokina, "Zherty goloda 1933 g.—Skol'ko ikh?" *Istoriia SSSR* (1991), no. 5: 18–26; N. A. Ivinskii, "Golod 1932–33 godov: Kto vinovat?" *Golod 1932–33 godov: Sbornik statei*, Iu. N. Afanas'ev, ed. (Moscow, 1995), 64–65. A summary of early estimates is available in an exceptionally useful bibliographic article: Dana Dalrymple, "The Soviet Famine of 1932–34," *Soviet Studies* 14 (January 1964): 250–84. For the famine in the context of the demographic turmoil of the 1930s, see S. G. Wheatcroft and R. W. Davies, "Population," in *The Economic Transformation of the Soviet Union, 1913–1945*, Davies, Mark Harrison, and Wheatcroft, eds. (Cambridge, 1994), 67–69.

Disease typically accounts for a large share of famine-related deaths; see Amartya Sen, *Poverty and*

exceeded even its gruesome death toll. It marked the final victory of central Soviet authorities over the peasantry.⁶ Yet, for decades, only Ukrainian émigré groups devoted significant attention to the famine. In official Soviet histories, meanwhile, the famine remained a “blank spot,” described blandly as “difficulties on the grain-requisition front.”⁷

What explains this silence? How could such a massive catastrophe provoke only ripples of concern among Western observers? Soviet efforts to cover up the famine come as no great surprise. Yet Western observers, in spite of widespread interest in the Soviet Union, wrote little of the famine. Many explanations for the lack of Western coverage focus on the ideological inclinations of two American journalists who denied the extent of the famine: Walter Duranty of the *New York Times* and Louis Fischer of *The Nation*. Two fellow journalists—William Henry Chamberlin (*Christian Science Monitor* and *Manchester Guardian*) and Eugene Lyons (United Press wire service)—were among the first to blame this pair of reporters. With all the vitriol of ex-believers, Chamberlin and Lyons attributed the lack of news about the famine to what one called “the Stalinist Penetration of America.”⁸ Writers following Chamberlin and Lyons typically assert that Duranty and Fischer were accomplices in genocide who denied the famine for ideological reasons. Later critics have also made more explicit assertions that Soviet payoffs ensured the reporters’ cooperation.⁹

Famines: An Essay on Entitlements (Oxford, 1981), 203–06. For physiological and epidemiological perspectives, see Helen Young, “Nutrition, Disease and Death in Times of Famine,” *Disasters* 19 (1995): 94–109; and Ancel Keys, et al., *The Biology of Human Starvation*, 2 vols. (Minneapolis, 1951), 2: 1002–50.

⁶ On this point, see especially D’Ann Penner, “The Agrarian ‘Strike’ of 1932–1933,” Kennan Institute for Advanced Russian Studies, Occasional Papers, no. 269 (1998)—this is the most important English-language work on the causes, course, and consequences of the famine. See also Sheila Fitzpatrick, *Stalin’s Peasants: Resistance and Survival in the Russian Village after Collectivization* (Oxford, 1994); Moshe Lewin, “‘Taking Grain’: Soviet Policies of Agricultural Procurement before the War” (1974), and “The Kolkhoz and the Russian Muzhik” (1980), both in Lewin, *The Making of the Soviet System: Essays in the Social History of Interwar Russia* (New York, 1985); and Lynne Viola, *Peasant Rebels under Stalin: Collectivization and the Culture of Peasant Resistance* (Oxford, 1996).

⁷ Books by Ukrainian émigrés include *The Black Deeds of the Kremlin: A White Book*, 2 vols. (Detroit, 1953–55); *The Great Famine in Ukraine: The Unknown Holocaust* (Jersey City, N.J., 1983); and Walter Dushnyk, *50 Years Ago: The Famine Holocaust in Ukraine: Terror and Human Misery as Instruments of Soviet Russian Imperialism* (New York, 1983). Distinguished Russian historian I. E. Zelenin applied the term blank spots (*belye piatna*, literally “white spots”) specifically to collectivization efforts (1928–33) and the ensuing famine; see Zelenin, “O nekotorykh ‘belykh piatnakh’ zavershaiushchego etapa sploshnoi kollektivizatsii,” *Istoriia SSSR* (1989), no. 2: 3–19.

⁸ Eugene Lyons, *The Red Decade: The Stalinist Penetration of America* (1941; rpt. edn., New Rochelle, N.Y., 1971), 122–24. Lyons arrived in Moscow in 1927, fresh from an assignment from TASS, the official Soviet news bureau, and determined to “bore from within” the capitalist system by working for a “bourgeois” news agency. Chamberlin’s interest in Russia dated back to a stay in Greenwich Village in the early 1920s, when many Village radicals followed Russian events enthusiastically. The transformation from radical to conservative was common among interwar intellectuals in America; see, for example, John P. Diggins, *Up from Communism: Conservative Odysseys in American Intellectual History* (New York, 1975); and, with more *simpatico*, Judy Kutulas, *The Long War: The Intellectual People’s Front and Anti-Stalinism, 1930–1940* (Durham, N.C., 1995).

⁹ James E. Mace, “The American Press and the Ukrainian Famine,” in *Genocide Watch*, Helen Fein, ed. (New Haven, Conn., 1992), 121; Mace, “The Politics of Famine: American Government and Press Response to the Ukrainian Famine, 1932–33,” *Holocaust and Genocide Studies* 3 (1988): 75–94; M. Wayne Morris, *Stalin’s Famine and Roosevelt’s Recognition of Russia* (Lanham, Md., 1994), 94–95; James William Crowl, *Angels in Stalin’s Paradise: Western Reporters in Soviet Russia, 1917 to 1937, a Case Study of Louis Fischer and Walter Duranty* (Lanham, 1982), 142, 158.

This ideological critique of famine coverage is typically linked to a political interpretation of the famine itself. Politics infused the émigré Ukrainians' writings on the famine, as well as the first widely read history of the famine, Robert Conquest's *Harvest of Sorrow* (1986). These works blamed the famine on politics, typically claiming that Soviet leaders planned the famine to satisfy genocidal desires to punish the Ukrainians for their nationalist aspirations.¹⁰ More recent works on the famine, however, have explained the famine in economic terms, as the final battle in a drawn-out war over grain harvests. Based on masses of newly available materials from both local and central archives, these new writings in no way excuse central government officials. Yet they place less emphasis on advance planning.¹¹ Such well-researched local studies, primarily by historians in post-Soviet states, have come to shape a new paradigm for understanding the famine.¹² According to this emerging paradigm, the famine marked a major battle in a drawn-out war between the Soviet government and the peasants for control of the grain grown by the peasantry. The government's plans for rapid industrialization required grain for exports (to purchase foreign machinery) and domestic consumption (to feed industrial workers). The impressive evidence unearthed by these scholars demon-

¹⁰ The most widely read work by those arguing genocide is Robert Conquest, *The Harvest of Sorrow: Soviet Collectivization and the Terror-Famine* (Oxford, 1986); see also U.S. Commission on the Ukrainian Famine, *Investigation of the Ukrainian Famine, 1932–1933: Report to Congress* (Washington, D.C., 1988), which was staffed by James E. Mace, who had served as Conquest's "junior collaborator" on *Harvest of Sorrow*; and Conquest, *et al.*, *Man-Made Famine in Ukraine* (Washington, 1984).

¹¹ My emphasis on economic issues has been informed by Shtefan [Stephan] Merl, "Golod 1932–1933 godov—Genotsid ukraintsev dlia osushchestvleniia politiki rusifikatsii?" *Otechestvennaia istoriia* (1995), no. 1: 49–61; and Stephan Merl, "War die Hungersnot von 1932–1933 eine Folge der Zwangskollektivierung der Landwirtschaft oder wurde sie bewusst im Rahmen der Nationalitätspolitik herbeigeführt?" *Ukraine: Gegenwart und Geschichte eines neuen Staates*, Guido Hausmann and Andreas Kappeler, eds. (Baden-Baden, 1993). Merl's detailed critiques are in basic agreement with Russia's leading agricultural historian of the Soviet period, Viktor Petrovich Danilov, and his students and colleagues. See, for instance, V. P. Danilov and N. V. Teptsova, "Kollektivizatsiia: Kak eto bylo," *Pravda*, August 26, 1988, and September 15, 1988; Ivnitskii, "Golod 1932–33 godov"; and I. E. Zelenin, N. A. Ivnitskii, V. V. Kondrashin, and E. N. Oskolkov, "O golode 1932–33 godov i ego otsenke na Ukraine," *Otechestvennaia istoriia* (1994), no. 6: 256–62.

Other scholars have explained the famine as the result of poor weather and military needs. See Mark B. Tauger, "The 1932 Harvest and the Famine of 1933," *Slavic Review* 50 (Spring 1991): 70–89; see also the bitter exchange between Conquest and Tauger in *Slavic Review* 51 (Spring 1992): 192–94. Also R. W. Davies, M. B. Tauger, and S. G. Wheatcroft, "Stalin, Grain Stocks, and the Famine of 1932–1933," *Slavic Review* 54 (Fall 1995): 642–57.

¹² The overall scope of the famine is discussed in E. A. Osokina, *Ierarkhiia potrebleniia: O zhizni liudei v usloviakh stalinskogo snabzheniia 1928–1935 gg.* (Moscow, 1993), chap. 2. On Russia, see V. V. Kondrashin, "Golod 1932–33 godov v derevne Povolzh'ia" (Candidate's dissertation, Institute of Soviet History, Soviet Academy of Sciences, 1991)—summarized in an article with a similar title in *Voprosy istorii* (1991), no. 6: 176–81; E. N. Oskol'kov, *Golod 1932/1933: Khlebozagotovki i golod 1932/33 goda v Severno-kavkaznom krae* (Rostov, 1991); and Penner, "Agrarian 'Strike' of 1932–33." Work on the famine in Ukraine is more voluminous; see especially *Kolektivizatsiia i holod na Ukraini, 1929–1933: Zbirnyk dokumentiv i materialiv*, H. M. Mykhailychenko and E. P. Shatalina, comps., S. V. Kul'chyts'kyi, *et al.*, eds. (Kiev, 1992); and *Holodomor 1932–33 rr. v Ukraini: Prychyny i naslidky*, S. V. Kul'chyts'kyi, ed. (Kiev, 1995). On Kazakhstan, where the famine was connected with "sedentarization" of nomad groups, see Zh. B. Abylkhozin, M. K. Kozybaev, and M. B. Tatimov, "Kazakhstanskaia tragediia," *Voprosy istorii* (1989), no. 7: 53–71.

Excellent overviews on the peasant war are D'Ann Rose Penner, "Pride, Power and Pitchforks: A Study of Farmer-Party Interactions on the Don, 1920–1928" (PhD dissertation, University of California, Berkeley, 1995); Penner, "Stalin and the Italkanka of 1932–33 in the Don Region," *Cahiers du monde russe* 39 (1998): 27–67; and Andrea Graziosi, "The Great Soviet Peasant War: Bolsheviks and Peasants, 1917–1933," *Harvard Papers in Ukrainian Studies* (1996).

strates the importance of both economic and political factors, as well as the extraordinary efforts of the peasants to stand up for their own interests. No similarly compelling evidence exists, by contrast, to support political interpretations focusing on ethnic genocide. The 1932–1933 famine was not a process driven primarily by desires to carry out ethnic genocide. Rather, the catastrophe resulted from the Soviet leadership's larger struggle to transform an agrarian empire into an industrial power.

Contrary to political interpretations of the famine coverage, contemporary accounts often recognized that rapid modernization was a core element of the famine itself. The fact that the famine did not provoke a major response in the West had less to do with individual politics and individual perfidy than with well-worn Western understandings of Russia. Some reputable American newspapers, after all, printed accurate and disturbing reports of the famine. To answer the questions about the weak American response to the famine, then, involves consideration of broader issues than political beliefs alone. Duranty and Fischer used language and conceptual frameworks similar to their accusers'. "National-character" stereotypes, first, led many experts to expect little from the Russian peasants—little, that is, aside from passivity, conservatism, and apathy.¹³ Many experts, secondly, expressed their appreciation of the Soviet Union's program to modernize rapidly and develop its long—"backward" economy.¹⁴ Finally, the widespread belief that Russian industrialization would entail high human costs drew on notions of Russian and/or "Asiatic" character traits.¹⁵

Previously unexamined documents, from both Soviet and American sources,

¹³ Especially given the prevalence of ethnic interpretations of this famine, it is worth noting in passing that few of the observers in the 1920s and 1930s distinguished between Ukrainian and Russian "character traits." On Russian stereotypes of the peasantry, see especially Cathy A. Frierson, *Peasant Icons: Representations of Rural People in Late 19th Century Russia* (Oxford, 1993). The origins of Western stereotypes of Russians are beyond the scope of this article—see Larry Wolff, *Inventing Eastern Europe: The Map of Civilization on the Mind of the Enlightenment* (Stanford, Calif., 1994), for early uses of these categories. Such stereotypes were dominant in late nineteenth-century French scholarship on Russia, scholarship widely read in the United States in both French and English; most influential in the United States were Anatole Leroy-Beaulieu, *The Empire of the Tsars and the Russians*, Zenaide A. Ragozin, trans., 2 vols. (New York, 1893–96); and Alfred Rambaud, *The History of Russia from the Earliest Times to 1877*, Leonora B. Lang, trans. (New York, 1878). For these authors in context, see Martha Helms Cooley, "Nineteenth-Century French Historical Research on Russia—Louis Leger, Alfred Rambaud, Anatole Leroy-Beaulieu" (PhD dissertation, Indiana University, 1971). British authors making similar claims were also widely read in the United States: E. B. Lanin [pseud. of E. J. Dillon], *Russian Traits and Terrors: A Faithful Picture of the Russia of To-day* (Boston, 1891); and Donald MacKenzie Wallace, *Russia*, 2 vols. (New York, 1877). On American reception, see Norman E. Saul, *Concord and Conflict: The United States and Russia, 1867–1914* (Lawrence, Kans., 1996), esp. 183–84. Important connections between "racial" stereotypes and development are outlined in Michael Adas, *Machines as the Measure of Man: Science, Technology and Ideologies of Domination* (Ithaca, N.Y., 1989).

¹⁴ Once again, the literature on this topic is huge. On Russian visions of economic transformation, see especially Esther Kingston-Mann, *In Search of the True West: Culture, Economics, and Problems of Russian Development* (Princeton, N.J., 1999); George Yaney, *The Urge to Mobilize: Agrarian Reform in Russia, 1861–1930* (Urbana, Ill., 1982). On American notions, see especially John M. Jordan, *Machine-Age Ideology: Social Engineering and American Liberalism, 1911–1939* (Chapel Hill, N.C., 1994). Social transformation in the name of modernization is the subject of an ambitious and provocative work that examines key moments of "authoritarian high modernism": James Scott, *Seeing Like a State: Why Certain Schemes to Improve the Human Condition May Fail* (New Haven, Conn., 1998).

¹⁵ While many historians have noted (primarily in passing) the prevalence of Western claims of Russia's "non-European" or "Asiatic" nature, fewer have explored the political and intellectual implications of these claims; see Wolff, *Inventing Eastern Europe*; Anders Stephanson, *Kennan and the*

reveal how much Duranty's and Fischer's writings and activities had in common with those of their principal accusers, Lyons and Chamberlin. The accusers wrote their angriest works, sharply condemning Duranty and Fischer, shortly after their respective breaks with Soviet Communism, coming on the heels of the 1932–1933 famine. Most subsequent attacks on the famine coverage have taken their evidence, tone, and even their book titles from these early works.¹⁶ But all four journalistic protagonists-cum-antagonists (Chamberlin, Duranty, Fischer, and Lyons) invoked stereotypes of Russian character and expressed their belief in economic development. Carefully tracing the actions and private writings of these journalists in 1932 and 1933 necessitates examining a wider range of evidence than the few frequently cited pieces for which each journalist is best known. These materials reveal that Chamberlin and Lyons shared with their nemeses Duranty and Fischer common assumptions about the need for “modernization” as well as notions of Russian “national character.” This statement should not in any way exonerate any of the journalists for disingenuous and even dishonest reporting. While Duranty and Fischer have been rightly attacked for their misleading coverage of the famine, the assumptions underlying their articles bore many similarities to those of their critics. Uncovering these common aspects does not free any reporters from blame, but it does allow a consideration of the mental frameworks behind American understandings of the Soviet Union.

BEFORE EXAMINING THE INTELLECTUAL ISSUES shaping reports of the famine, however, more concrete limits on coverage deserve brief mention. The Press Office of the People's Commissariat for Foreign Affairs (known by its Russian acronym, NKID) worked assiduously to restrict foreign coverage of Soviet events. The four censors who staffed the Press Office had excellent credentials. They all spoke and read English (as well as French and/or German) well enough to prevent foreign reporters from carrying off many linguistic sleights-of-hand. Censors had to approve every dispatch that journalists wished to telegraph to their home offices. Such direct censorship did not apply to reports sent out by mail, however, and the time-honored tradition of sending letters out of Russia with westward-bound travelers provided yet another avenue to get dispatches to the United States.¹⁷ Yet such maneuvers did not escape the Press Office's powers of surveillance. Thanks to detailed analyses of press reports conducted by Soviet embassies in London, Paris,

Art of Foreign Policy (Cambridge, Mass., 1989), chap. 1. The literature around Edward Said, *Orientalism* (New York, 1979), is of course relevant here.

¹⁶ Crowl's title (*Angels in Stalin's Paradise*), for instance, is quoted from Lyons, *Red Decade*, 93. S. J. Taylor, *Stalin's Apologist: Walter Duranty, the New York Times's Man in Moscow* (Oxford, 1990) uses some of the same language, but incorporates much more research and a somewhat more balanced tone than Crowl.

¹⁷ These general comments about Soviet press censorship are based primarily on the materials in the foreign-ministry archive, scattered throughout various collections. Archival staff indicated in the spring of 1995 that the earliest documents in the Press Office collection date only to 1943. Other context comes from the discussions in memoirs and other writings by the office's principal “clientele,” Western journalists themselves. A thorough, though dated, list of reporters is available in U.S. Department of State, Division of Library and Reference Services, *American Correspondents and Journalists in Moscow, 1917–1952: A Bibliography of Their Books on the USSR*, Bibliography no. 73 (March 27, 1953).

and Berlin, as well as the “information office” in Washington (not an embassy until 1934), the Press Office staff received summaries of news articles that had not been vetted through their office. If these reports sufficiently concerned the NKID, it intervened either directly or indirectly to discipline the offending journalist. Soviet officials also exploited their contacts with American editors, including those at the *New York Times* and the United Press, to lobby for a different slant on coverage or, on at least one occasion, for a change of reporters.¹⁸ The Press Office could also delay or deny outright a journalist’s request for a visa.¹⁹

In spite of all of these tools to control foreign journalists, however, Westerners spread rumors about an impending famine through Moscow’s journalistic community in the summer of 1932. One Harvard political scientist well acquainted with the Moscow-based journalists wrote a friend in the State Department that “there is definite famine” in Ukraine.²⁰ Other information about rural conditions arrived overseas without first passing through the Moscow colony. German embassy officials reported famine conditions in grain-growing regions of the USSR in the summer of 1932. One published report, based more on Soviet statistics than on firsthand experience, referred to “famine [*Hungersnot*] in the fullest sense of the word” in Ukraine, the Lower Volga, western Siberia, and Kazakhstan.²¹ German agricultural attaché Otto Schiller, one of the best-informed foreigners in Moscow, spent much of 1932 touring the Soviet countryside. Traveling with Canadian Andrew Cairns, Schiller detailed the dire conditions in the Soviet countryside in an article that appeared in Germany in February 1933. Cairns’s reports reached the British Foreign Office even earlier.²²

¹⁸ For example, memoranda of conversation with Edwin James of the *New York Times* and Karl A. Bickel of the United Press are in (respectively) Podol’skii diary, November 3, 1930, Arkhiv vneshnei politiki Rossiiskoi Federatsii (hereafter, AVPRF), fond 0129 (Referantura po SShA), opis’ 13, papka 127, delo 319, list’ia 6–7 (hereafter, f./op./pap./d./ll.); Oumansky to NKID Collegium, January 1, 1933, AVPRF, f. 0129, op. 16, pap. 128a, d. 335, ll. 21–22. Bickel, the director of the United Press (UP) syndicate, was in Moscow to renegotiate the UP agreement with TASS; see Joe Alex Morris, *Deadline Every Minute: The Story of the United Press* (Garden City, N.J., 1947), 189.

¹⁹ Two prominent cases familiar to the subjects of this article were Paul Scheffer of *Berliner Tageblatt* and freelancer Maurice Hindus. On Scheffer, see Louis Fischer, “The Case of Paul Scheffer,” *Nation* 132 (August 31, 1932): 195–96; Scheffer, *Seven Years in Soviet Russia (With a Retrospect)*, Arthur Livingston, trans. (London, 1933), vii–xvi; Bogdanov to Stomoniakov, September 25, 1930, AVPRF, f. 0129, op. 13, pap. 127, d. 13, l. 1; and Kagan diary excerpt, July 16, 1932, AVPRF, f. 0129, op. 15, pap. 128, d. 328, l. 41. On Hindus, see Gnedin to Oumansky, October 23, 1937, and November 27, 1937; also Astakhov to Oumansky, April 2, 1937—all in AVPRF, f. 0129, op. 20, pap. 133a, del. 342, ll. 5, 20, 24–24ob., 32.

²⁰ Bruce C. Hopper to Robert F. Kelley, July 24, 1932, in Box 5, Division of East European Affairs Records, State Department Records, Record Group 59, U.S. National Archives; a similar letter appears in Box 35, Hamilton Fish Armstrong Papers, Mudd Library, Princeton University.

²¹ Otto Auhagen, “Wirtschaftslage der Sowjetunion im Sommer 1932,” *Osteuropa* 7 (August 1932): 644–55 (quoted at p. 645). The American “listening post” in Riga reported this article to Washington in Skinner to Secretary of State, November 15, 1932, 861.6131/261, SDDE. Auhagen was a former agricultural adviser at the German embassy in Moscow who left to direct the Osteuropa Institut in Breslau, under whose auspices *Osteuropa* was published; see *Red Economics*, Gerhard Dobbert, ed. (Boston, 1932), iii; Jutta Unser, “‘Osteuropa’—Biographie einer Zeitschrift,” *Osteuropa* 25 (September 1975): 562–63.

²² Otto Schiller, “Die Krise der sozialistischen Landwirtschaft in der Sowjetunion,” *Berichte über Landwirtschaft* 79, Sonderheft (1933). The Soviets viewed Schiller’s and Auhagen’s writings as “impudent and undisguised espionage”: Vinograd to D. G. Shtern, n.d., AVPRF, f. 05 (Sekretariat Litvinova), op. 13, pap. 90, d. 14, ll. 87–87ob. Cairns’s reports are reprinted in full as Andrew Cairns, *The Soviet Famine, 1932–33: An Eye-witness Account of Conditions in the Spring and Summer of 1932*,

More details about rural conditions reached Moscow in the fall of 1932. One British diplomat reported in late October that Duranty “ha[d] at last awakened to the agricultural situation,” blaming the severe problems on shortages of labor and draft-power. The diplomat summarized Duranty’s analysis: “There are millions of . . . peasants whom it is fairly safe to leave in want . . . [But] is there no limit to people’s endurance?” Yet Duranty did not foresee any organized resistance. His articles typically came across as somewhat sanguine, noting that the USSR was “in better shape than most of the world,” in spite of serious supply problems that had sapped “peasant energy and initiative.” Even an otherwise celebratory article on the fifteenth anniversary of Bolshevik rule closed with mixed optimism: “Times are hard and will not be easy in the near future.” The ultimate victory of “socialist building,” though, was assured.²³

Duranty’s later reports took a markedly less optimistic tone about the situation in Russia. At the end of November, he published a six-part series on the food shortage, carried out of Moscow to evade censorship. This series established the parameters for Duranty’s subsequent writings on the situation. While he dismissed the predictions of famine (“there is no famine or actual starvation, nor is there likely to be”), Duranty did write of the “great and growing food shortage in town and country alike,” which was having “ever graver” effects. Only bread was available in reasonable amounts. Dairy products were never seen. Meat and fish appeared only rarely and in quantities “below the people’s wants and probably below their needs.” The Russians’ capacity for sacrifice, however, would carry them through: “Russians have tightened their belts before to a far greater extent than is likely to be needed this Winter.” Duranty seemed impressed with Soviet leaders who were “not in the least trying to minimize [the food shortage’s] gravity, its widespread character and its harmful effects” but were not “much alarmed by it.” Finally, perhaps to explain his own reluctance to stray from Moscow, Duranty dismissed the need for a foreign observer to tour the villages, “where it commonly happens that disgruntled or disaffected elements talk loudest while others are busy working.”²⁴

Tony Kuz, ed. (Edmonton, 1989). German information was also available from the consulates in Kiev and Kharkov—see the reports filed in *Der ukrainischer Hunger-Holocaust: Stalins verschwiegener Völkermord 1932/33 an 7 Millionen ukrainischer Bauern im Spiegel geheimgehaltener Akten des deutschen Auswärtigen Amtes*, D. Zlepko, ed. (Sonnenbühl, 1988). Other reports reached Western Europe via the Italian consulates; see Andrea Graziosi, “‘Lettres de Kharkov’: La famine en Ukraine et dans le Caucase du Nord à travers les rapports des diplomates italiens, 1932–1934,” *Cahiers du monde russe et soviétique* 30 (1989): 5–106; a selection is also published in U.S. Commission on the Ukrainian Famine, *Investigation of the Ukrainian Famine*, appendix 2.

²³ Conversation between William Strang and Walter Duranty, October 31, 1932, in *The Foreign Office and the Famine: British Documents on Ukraine and the Great Famine of 1932–1933*, Marco Carynnyk, Lubomyr Y. Luciuk, and Bohdan S. Kordan, eds. (Kingston, Ont., 1988), 204. On British diplomats’ distant attitude toward the famine, see Michael Hughes, *Inside the Enigma: British Officials in Russia, 1900–1939* (London, 1997), 243–45. Walter Duranty, “Soviet in 16th Year; Calm and Hopeful,” *New York Times* (hereafter, *NYT*), November 13, 1932; Duranty, “Fifteen Stern Years of Soviet Rule,” *NYT Magazine*, November 6, 1932.

²⁴ Walter Duranty, “All Russia Suffers Shortage of Food,” *NYT*, November 25, 1932; Duranty, “Food Shortage Laid to Soviet Peasants,” *NYT*, November 26, 1932; Duranty, “Soviet Press Lays Shortages to Foes,” *NYT*, November 27, 1932; Duranty, “Soviet Not Alarmed over Food Shortage,” *NYT*, November 28, 1932; Duranty, “Soviet Industries Hurt Agriculture,” *NYT*, November 29, 1932; Duranty, “Bolsheviki United on Socialist Goal,” *NYT*, November 30, 1932.

The series served Duranty well in New York, where editors praised it as “one of the best stories current.” Yet it served him less well in Moscow, as a British diplomat reported: “Shortly [after the series appeared], Duranty was visited by emissaries from governing circles here (not from the Censorship Department of the People’s Commissariat for Foreign Affairs but from higher spheres) who reproached him with unfaithfulness . . . [D]id he not realize that the consequences for himself might be serious. Let him take this warning. Duranty, who was to have left for a short visit to Paris that day, put off his departure to wait further developments . . . He affects to think it possible that . . . he may not be allowed to return.”²⁵ Duranty postponed his departure, but left in early December.

Among his other activities in Paris, Duranty spoke to the Travellers Club. An American diplomat in attendance summarized Duranty’s views as follows: “The chief reason for his pessimism was the growing seriousness of the food shortage. This he ascribed to difficulties which the Government was having with its scheme of collective farming . . . He described the situation in Russia to-day as comparable to that which existed in Germany during the latter part of the war, when . . . the civil population was living on practically starvation rations.” According to internal reports, the Paris speech angered Soviet authorities.²⁶

By the end of 1932, then, Duranty had set a pattern for describing the rural crisis. He frequently employed military terminology, implying the need to stay above the fray. He issued critical and pessimistic reports on the food situation accompanied by denials that “famine conditions” existed. This pattern would continue throughout the famine and beyond.

As Walter Duranty published his November series on the food shortage in the Soviet Union, Louis Fischer voiced few worries about Soviet conditions: “I feel as if this were the beginning of the end of a long Soviet winter which has lasted several years. Now the earth commences to smell of spring.” Perhaps the new springtime provoked Fischer’s allergies, since he left Moscow for an extended American tour—December until the following June. His final article from Moscow called for easing the pressure on Soviet peasants. It also noted a decline in grain collections in the North Caucasus region, blaming “bad organization, slack guidance by party members [and] insufficient loyalty to Moscow’s instructions.” The problems might extend even farther, as “important grain-growing areas like the Ukraine, North Caucasus, the Volga region and the central black-earth district” had no grain for

²⁵ Markel to Edwin L. James, November 18, 1932, and November 22, 1932, both on reel 32, Edwin L. James Papers, *New York Times* Archive. William Strang to Laurence Collier, December 6, 1932, in Carynnyk, *Foreign Office and the Famine*, 209–10.

²⁶ Enclosure 1 with Walter Edge to Secretary of State, December 10, 1932, 861.5017 Living Conditions/572, SDDF. The NKID Press Office was already wary of Duranty prior to his Paris trip, presumably because of his articles on the food shortages: Podol’skii to Rozenberg, November 29, 1932, AVPRE, f. 0129, op. 15, pap. 128, d. 328, l. 82. A Latvian diplomat in Moscow later reported that Duranty was “no longer regarded as a friend of the Bolsheviks” in the fall of 1932; Felix Cole to Secretary of State, April 8, 1933, 861.5017 Living Conditions/671, SDDF. Duranty frequently compared even the most dire Soviet circumstances favorably to what he saw as a reporter during World War I; see, for instance, “About the Author” in Walter Duranty, *One Life, One Kopeck* (New York, 1937), which records that Duranty’s wartime service was “such a baptism of fire that nothing he saw afterwards in the Soviet Union made him turn a hair.”

open sale. Fischer thus identified food shortages but only in cryptic phrases containing gross understatements.²⁷

Like Fischer, journalist William Henry Chamberlin also left Moscow for an extended trip to the United States, perhaps spurred by the rumors about food shortages. Chamberlin predicted food supply problems for the fall and winter of 1932–1933. In early October, he recommended to his replacement that foreigners should consider hoarding nonperishable food for what promised to be a tough winter.²⁸ Traveling through London en route to the United States, Chamberlin gave a standing-room-only talk at the Royal Institute of International Affairs. The overall tone of the speech was quite positive. Chamberlin lauded the growing strength of the Red Army and criticized those who opposed American recognition of the Soviet Union. He sounded decidedly optimistic about the economic prospects for the Soviet Union: collectivization had exacted a substantial toll but was making progress. In any case, he concluded, it could no longer be reversed. He hesitated to predict the future in the Soviet countryside but suggested that recent Soviet measures with regard to trade and consumer goods would determine the success or failure of the effort. On the other hand, Chamberlin also warned that a “dual agrarian and food crisis” would be costly in human and financial terms. He shrank from calculating the bottom line on the Five-Year Plan’s impact: “It is very difficult to make any sort of arithmetical balance sheet of how much happiness and unhappiness this period of violent and great change has brought in Russia.” A Soviet report summarized the talk with apparent relief: Chamberlin “behaved entirely favorably for the USSR. In fact, in a few cases he resorted to quite original forms of defense of the USSR.” Chamberlin also submitted an article to a British magazine; that article praised the “impressive addition to the national industrial capital” but noted that it “has been purchased at an extremely high price in the standard of living.”²⁹

By New Year’s Day 1933, then, both Chamberlin and Duranty had given mixed reports on Soviet conditions. They both remained optimistic about Soviet industrialization efforts while also describing the costs involved. Fischer, by contrast, expressed nothing but optimism and enthusiasm for the coming year. While talk of a “crisis” appeared in Chamberlin’s and Duranty’s writings, neither journalist considered the situation a famine *per se*.

²⁷ Louis Fischer, “Fifteen Years of the Soviets,” *Nation* 135 (November 23, 1932): 495; Fischer, “Stalin Faces the Peasant,” *Nation* 136 (January 11, 1933): 39–41.

²⁸ Diary entry, October 4, 1932, Malcolm Muggeridge Diary, Hoover Institution Archives, Stanford University. Chamberlin may have heard from German attaché Otto Schiller, whom he called, in a 1968 interview, one of his four closest friends in Moscow; see Robert H. Myers, “William Henry Chamberlin: His Views of the Soviet Union” (PhD dissertation, Indiana University, 1973), 54–55. On the speaking tour, see William Henry Chamberlin, *Confessions of an Individualist* (New York, 1940), 154. One biographer speculates that perhaps his “aversion to carnage” led him to leave Moscow: Michael Samerdyke, “Explaining the Soviet Enigma: William Henry Chamberlin and the Soviet Union, 1922–1945” (MA thesis, Ohio University, 1989), 72.

²⁹ The Royal Institute talk appeared as William H. Chamberlin, “What Is Happening in Russia?” *International Affairs* (London) 12 (March 1933): 187–205. Soviet impressions of the talk seem slightly optimistic in comparison with the published version: “Vypiska iz dnevnika press-attashe polpredstva SSSR v Anglii Tolokonskogo,” November 23, 1932, AVPRF, f. 0129, op. 15, pap. 128, d. 328, ll. 11–12; and Tolokonskii to Otdel Pechati, December 3, 1932, AVPRF, f. 05, op. 12, pap. 82, d. 15, ll. 99–103. See also Chamberlin, “Impending Change in Russia,” *Fortnightly Review*, n.s. 139 (January 1, 1933): 10.



Some of the foreign journalists based in Moscow traveled to Central Asia for the opening of the Turkestan-Siberian Railroad extension in 1930. This group includes William Henry Chamberlin (crouching, right of center), Louis Fischer (behind Chamberlin and to the right), and Eugene Lyons (behind and to the right of the man holding a camera). Photo courtesy of Carl and Elizabeth Krumpe.

INDICATIONS OF ACTUAL FAMINE first appeared in the mainstream Western press in early 1933, spurred by two reports from the countryside. One set of reports came from Malcolm Muggeridge, a Briton then working as Chamberlin's substitute for *Manchester Guardian* coverage. Muggeridge arrived in Moscow in the fall of 1932, full of enthusiasm for Soviet ideals. It quickly dissipated. In spite of his dislike of most foreign journalists in Moscow, Muggeridge repeated their national-character clichés. After hearing of starvation in Kiev, for instance, Muggeridge remarked in his diary that "starvation is in the nature of things" for a Russian. He also attempted to use his "Eastern" experiences—in India—to understand Russia. In both places, he wrote, "mere brutality . . . [is] not in and of [itself] a condemnation" of either British colonial or Soviet government policy.³⁰

Muggeridge sent reports on famine conditions to the *Manchester Guardian* in early 1933. His first leads on the famine came from an anonymous visitor who deposited articles from provincial newspapers on the reporter's doorstep and also from Dr. Joseph Rosen, an American organizing Jewish agricultural settlements in the USSR. At the end of January, the reporter traveled to Ukraine and the North Caucasus to observe conditions firsthand. The *Manchester Guardian* did not print Muggeridge's dispatches until late March 1933, perhaps because they jarred so

³⁰ Diary entries for September 16 and 28, 1932, Muggeridge Diary; John Bright-Holmes, introduction, *Like It Was: The Diaries of Malcolm Muggeridge* (London, 1981), 13.

sharply with the newspaper's generally favorable editorial stance toward the USSR. The *Guardian's* three-part series reported on "famine conditions" in the North Caucasus, conditions that Muggeridge contended would last at least three to five more months. It also described "hunger in the Ukraine" and the author's pessimistic predictions for the future. Muggeridge blamed heavy grain requisitions for the precarious situation: they had left the population with a "characteristic peasant look—half resignation and half cunning." While Western journalists in Moscow may follow attentively the experiment of collectivization, Muggeridge concluded, "for the participants, [it was] often more disagreeable than interesting."³¹

Eugene Lyons set into motion a second set of articles on the famine, this time appearing in American newspapers. His secretary first read of potential problems in the North Caucasus, in a local Soviet newspaper article about a secret-police "rampage" in a village near Rostov. This information set the tone for Lyons's dispatches of January and February 1933, which emphasized the food question and the harshness of government grain demands. Yet Lyons also characterized government repression as a response to peasant laziness. One undated dispatch adopted the Soviet government's viewpoint, applauding improved grain collections, while other dispatches noted the "intense struggle to extract seed grain . . . developing nationwide as the first act of the drama of spring sowing." Lyons reported dire conditions in Ukraine, the North Caucasus, and parts of the Lower Volga, but he maintained optimistically that these conditions were "not typical of the entire country." Soviet economic policy, Lyons wrote later that year, amounted to various efforts to "overcome peasant apathy."³² Lyons's view of peasant character traits—in which apathy played a central role—thus explained the hardships in the Soviet countryside.

While Lyons apparently did not write a dispatch on the Rostov news item, he did alert two American journalists, William Stoneman of the *Chicago Daily News* and Ralph Barnes of the *New York Herald Tribune*. Stoneman and Barnes quickly hired a translator and bought train tickets to Rostov to "view the performance," as Stoneman later worded it.³³ Stoneman's dispatch of February 6 described "virtual

³¹ Diary entries, December 1, 1932, January 4 and 11, 1933, Muggeridge Diary. On his trip to the countryside, see Muggeridge to Crozier [his editor at the *Manchester Guardian*], January 14, 1933, cited in Richard Ingrams, *Muggeridge—The Biography* (New York, 1995), 64. The articles were published in the *Manchester Guardian*: "Famine in North Caucasus," March 25, 1933; "Hunger in the Ukraine," March 27, 1933; and "Poor Prospects for Harvest," March 28, 1933. His reports were apparently delayed and toned down (he used the word "mangled") by his editors; see Marco Carynnyk, "The Famine the *Times* Couldn't Find," *Commentary* 76 (November 1983): 33. See also Ingrams, *Muggeridge*, 62–69; and David Ayerst, *The Guardian: Biography of a Newspaper* (London, 1971), 511–13.

³² Improvement—telegram 24142, folder 4, Box 28, Henry Shapiro Papers, Library of Congress; drama—telegram 10120, folder 7, Box 28; not hopeless—telegram 15134, folder 7, Box 28; apathy—telegram 12152, folder 8, Box 28. Shapiro was Lyons's successor with the United Press syndicate in Moscow. Unfortunately, none of the telegrams in these folders is dated.

³³ This narrative is reconstructed from chap. 5 of Stoneman's autobiography (dated March 1, 1967), Box 1, William Stoneman Papers, Bentley Library, University of Michigan; Stoneman interview with Whitman Bassow, November 10, 1984, Box 2, Whitman Bassow Papers, Library of Congress. Also see Lyons, *Assignment in Utopia*, 545–46; and Stoneman to Harrison Salisbury, May 16, 1979, cited in Taylor, *Stalin's Apologist*, 202, 235. Stoneman had always taken an interest in rural food supply, ending his first tour in Russia (in 1932) with reflections on localized shortages; see Edward Brodie to Secretary of State, February 24, 1932, 761.00/221, SDDF.

martial law” and increased activity of armed forces in the region despite the lack of collective resistance. He blamed the lack of grain in “one of Russia’s richest grain regions” on the central authorities’ “taking revenge on the peasants.” After a few days of observing conditions in Rostov and environs, the journalists were picked up by the local secret police and shipped back to Moscow. They nevertheless succeeded in smuggling reports to their newspapers. Ralph Barnes’s article focused on the terror in the Kuban’, mentioning the dire food situation there. Perhaps building on Duranty’s November reports, Barnes mentioned “only a limited number of cases of deaths due strictly to starvation” but admitted that there were “many deaths resulting from disease attacking constitutions seriously undermined by lack of sufficient food.”³⁴

After the first of these accounts appeared in February 1933, senior Soviet officials banned foreigners’ travel within the USSR. Foreign journalists learned of the measure at the end of February. While the Press Office was charged with primary enforcement of the new ban, its censors unsuccessfully opposed a blanket prohibition, arguing confidently that they could keep foreigners out of the problem areas without calling attention to the situation by announcing a formal prohibition. In a letter written to Premier Viacheslav Molotov, the censors argued against the travel ban:

The decision on a new arrangement for foreign correspondents’ movement in the territory of the Union [the USSR] without the permission of the militia will without any doubt be interpreted by Moscow-based correspondents, and also by the international press, as the denial of freedom of movement for foreigners/journalists for the purpose of hiding from them the “true situation” in the localities . . .

The negative consequences of a general ban on the free movement of foreign correspondents might be averted if the NKID Press Office, together with some general measures, could in each individual case try to obtain voluntary rejections of this or that trip which is undesirable to us. In precisely this way, two trips to Ukraine by foreign correspondents were recently prevented.³⁵

The Press Office staff protested the implementation of a full-fledged prohibition on travel, arguing that they could be just as effective in one-to-one conversations, convincing journalists not to visit afflicted areas without raising suspicions of a new policy.

Soviet officials, however, grossly overestimated their powers of persuasion. Stoneman (one of the two reporters alluded to in the final sentence quoted above) recounted his conversation with a censor in a manner that suggests the censors were heavy-handed, did much to arouse journalists’ suspicions, and in no way succeeded in obtaining a “voluntary” change of itinerary from Stoneman. The censor first

³⁴ William Stoneman, “Russia Clamps Merciless Rule on Peasantry,” *Chicago Daily News* [dispatch filed February 6, 1933], found after page 16 of Stoneman’s “Autobiography,” Box 1, Stoneman Papers. See also Stoneman, “Little Liberty Permitted Foreigner in Kuban Area,” *Chicago Daily News*, March 28, 1933; Stoneman, “Communists Find It Easy to Justify Peasant Exile,” *Chicago Daily News*, March 30, 1933; Ralph Barnes, “Soviet Terrorizes Famine Region by Night Raids for Hidden Grain,” *New York Herald-Tribune*, February 6, 1933. Barnes’s high regard for Duranty’s work might well suggest that Barnes may have adopted Duranty’s argument as his own; see Ralph Barnes to Joseph Barnes [no relation], June 4, 1932, Box 6, Joseph Barnes Papers, Columbia University Library.

³⁵ “Zapiska otdela pečati, poslannaia t. Molotovu,” February 25, 1933, AVPRF, f. 05, op. 13, pap. 90, d. 13, ll. 46–47.

questioned Stoneman's need to visit Ukraine as opposed to some other rural region. He then pleaded with the reporter "as a friend" before finally declaring that "you had better postpone your trip."³⁶

News of the travel ban spread quickly through the foreign colony in Moscow. The *New York Times* and other major newspapers, however, printed nothing on either the Stoneman/Barnes reports or the new travel restrictions for foreigners. Duranty, perhaps chastened by his troubles with Soviet authorities in December, changed the focus of his reportage. He shifted toward coverage of political events, stopping on economic conditions only long enough to predict a "decisive struggle on the agrarian front" in the spring. Duranty accentuated the poverty and "backwardness" of Russian peasants, comparing peasants not to farmers but to "farm-cattle" because of their passivity and servile mentality. He also framed the rural conflicts in military terms. "I am inclined to think," he concluded, that the Bolsheviks will defeat the peasants "in the long run, but it won't be easy." By constantly focusing on the future, Duranty did not deny peasants' hardships—in fact, he rather relished them—but he attributed them to peasant character. According to Duranty, the Bolsheviks needed to "swing all the forces in their command into an effort to overcome peasant apathy, individualism, dislike of novel collective methods and the previous mismanagement of collective farms." Prospects for the current harvest were poor, and the food shortage, "already widespread and serious," would only get worse. The picture looked bleak, especially given the peasants' degree of "degeneration and apathy."³⁷ Like Lyons, Duranty blamed peasant character, primarily apathy, for the problems with collectivization.

Chamberlin, whose November speech in London seemed relatively sanguine, apparently suffered a mood change while at sea. Once in the United States, he emphasized both the rising inequalities and the "food shortage and falling off in agricultural production" that were plaguing Russia. He did, however, find some reason for optimism: the most recent government policies, he believed, would alleviate the food situation.³⁸ He also published an article in *The New Republic* (a magazine at that time sympathetic to the Soviet cause) describing the Five-Year Plan as a "forced, concentrated drive for high speed industrialization, regardless of the cost to the daily standard of living." The article mentioned both domestic food shortages and rising grain exports. But prospects were good, Chamberlin claimed, because the Soviet leaders had realized that "the process which someone wittily described as 'starving itself great' can be and indeed has been pushed to a point where it is distinctly subject to a law of diminishing returns." In another article, Chamberlin noted the "considerable strides" the USSR had made "toward its goal of becoming a powerful industrial country." In spite of the hardships, especially for those groups targeted by the Soviets, the Five-Year Plan represented "Russia's

³⁶ "Conversation with Comrade Podolskii, chief Censor of Moscow Foreign office—Tuesday, February 23rd, 1933." Box 1, Stoneman Papers.

³⁷ Walter Duranty, *Duranty Reports Russia*, selected and arranged by Gustavus Tuckerman, Jr. (New York, 1934), 295 (dispatch dated January 29, 1933) [future citations will be page number (dispatch date)]. Duranty, "Russia's Peasant: The Hub of a Vast Drama," *Duranty Reports Russia*, 265, 274 (February 2, 1933), 304, 306 (February 27, 1933).

³⁸ "Russia Offers Inducements to Increase Farmer Output," *Christian Science Monitor*, December 21, 1932.

extraordinary contribution to economic history.” Chamberlin, like Duranty, described the high costs of Russian collectivization and industrialization but nevertheless endorsed the lessons it offered and the achievements it promised.³⁹

While Chamberlin and Fischer remained outside the reach of Soviet censors, Duranty and Lyons continued to report on poor living conditions in the Soviet Union from Moscow, and were thus prevented from explicitly mentioning famine. Duranty headed for another European vacation in early March, however, and filed dispatches not subject to direct Soviet censorship. These reports noted the “gloomy picture” in Ukraine as well as the North Caucasus and Lower Volga regions. The *New York Times* reporter saw a “brighter side.” Upon learning of new repressive organs (political departments of Machine-Tractor Stations located throughout the countryside), Duranty extolled them as “the greatest constructive step toward the efficient socialization of agriculture.” He blamed a familiar culprit for the food crisis. After one particularly critical assessment of Russian national character, Duranty concluded that “what is wrong with Russian agriculture is chiefly Russians.”⁴⁰

Muggeridge’s *Manchester Guardian* series was quickly followed by a report on the famine from Gareth Jones. Jones, a Russian-speaking assistant to former British Prime Minister David Lloyd George, obtained his information during brief travels through Ukraine. The articles described how starvation and disease had laid waste to whole villages in the region.⁴¹ These reports appeared within days of the stories by Stoneman and Barnes. Perhaps because Jones was not a permanent Moscow correspondent, the NKID singled him out for special treatment. The Press Office enlisted the help of the Moscow regulars in discrediting him. Lyons’s version of how Press Office chief Constantine Oumansky recruited the foreign journalists to “throw down Jones” has reached the status of a classic—even a cliché—in writings about famine coverage. As Lyons wrote in *Assignment in Utopia* (1937),

There was much bargaining in a spirit of gentlemanly give-and-take, under the effulgence of Umansky’s [*sic*] gilded smile, before a formula of denial was worked out. We admitted enough to soothe our consciences, but in roundabout phrases that damned Jones as a liar. The filthy business having been disposed of, someone ordered vodka and *zakuski* [snacks], Umansky joined in the celebration and the party did not break up until the early morning hours . . . He had done a big bit for Bolshevik firmness that night.⁴²

This text appears in almost every writing on the “famine cover-up” as proof positive of the journalists’ craven willingness to serve the Soviets. Yet outside evidence contradicts Lyons’s oft-told tale. First, there is some reason to doubt

³⁹ William H. Chamberlin, “Russia between Two Plans,” *New Republic* 74 (February 15, 1933): 7–8; Chamberlin, “Balance Sheet of the Five-Year Plan,” *Foreign Affairs* 11 (April 1933): 458, 466.

⁴⁰ Duranty, *Duranty Reports Russia*, 310–12 (March 2, 1933). On the political departments, see I. E. Zelenin, “Politotdely MTS—Prodolzhenie politiki ‘chrezvychaishchiny’ (1933–1934 gg.),” *Otechestvennaia istoriia* (1992), no. 6: 42–61.

⁴¹ “Famine in Russia—Englishman’s Story—What He Saw on a Walking Tour,” *Manchester Guardian*, March 30, 1933; Edgar Ansel Mowrer, “Russian Famine Now as Great as Starvation of 1921, Says Secretary to Lloyd George,” *Chicago Daily News*, March 29, 1933. Jones had worked with the leading British scholar of the Soviet Union, Bernard Pares; see Sir Bernard Pares, *A Wandering Student* (Syracuse, N.Y., 1948), 309–11.

⁴² Lyons, *Assignment in Utopia*, 576. While the Press Office chief’s name would today be transliterated as Konstantin Umanskii, he wrote his name as used in the text above.

Lyons's chronology. The meeting with the censors, he reported, took place after Jones's *Manchester Guardian* article appeared—therefore, after March 30, 1933. Lyons follows up his description of the gathering for “Bolshevik firmness” with a description of how each journalist was summoned to the Press Office and told not to leave Moscow without official permission. But Stoneman's account—corroborated by documents from American, British, and Russian archives—indicates that news of the ban circulated in late February.⁴³ Furthermore, no other Western correspondents—including both Duranty's assistant and Stoneman, who were present in Moscow and were later interviewed about the famine—ever mentioned this party. Lyons himself was rather sketchy on the details when asked about it years later. As his recollections were summarized by one historian,

Lyons remembers little more about the meeting with Oumansky than the description of it in *Assignment in Utopia*. It was not a “general session” of the foreign correspondents, he recalls, nor did Oumansky have to do more than “hint” as to what should be done. Lyons cannot remember who attended or even more specifically where the meeting was held. He adds, however, that “presumably” Duranty was there.⁴⁴

Whether or not this evening affair took place as Lyons described it, Duranty indeed did “throw down” fellow Briton Jones. In an article that remains a textbook example of double-speak, Duranty criticized Jones's judgment as “somewhat hasty” and based only on minimal travels in Ukraine. (Jones, it might be noted, undertook more travel than Duranty himself.) Duranty's article, published under the headline “Russians Hungry, but Not Starving,” cynically noted the number of times that foreigners have prematurely “composed the Soviet Union's epitaph.” Duranty derided Jones's most recent epitaph, claiming that Jones had “seen no dead or dying human beings” and therefore had little direct evidence of famine. Duranty did not deny the “deplorable” conditions, but he blamed the problems on the “novelty and mismanagement of collective farming.” In a justly infamous paragraph, Duranty then relied on his stock phrase and his usual military analogies: “But—to put it brutally—you can't make an omelette without breaking eggs, and the Bolshevik leaders are [like military commanders] . . . indifferent to the casualties that may be involved.” And a phrase just as infamous if less evocative, Duranty continued with his odd denial: “There is a serious food shortage throughout the country . . . There is no actual starvation or deaths from starvation, but there is widespread mortality from diseases due to malnutrition . . . In short, conditions are definitely bad in certain sections—the Ukraine, North Caucasus, and Lower Volga. The rest of the country is on short rations but nothing worse. These

⁴³ Sir Esmond Ovey to Foreign Office, March 5, 1933, in Carynnyk, *Foreign Office and the Famine*, 215; Sackett to Secretary of State, March 1, 1933, 861.5017 Living Conditions/595, SDDF, rpt. in M. Morris, *Stalin's Famine*, 170–81; “Zapiska ot dela pechati, poslannaia t. Molotovu,” February 25, 1933, AVPRF, f. 05, op. 13, pap. 90, d. 13, ll. 46–47.

⁴⁴ The party is not mentioned in Stoneman's “Autobiography” (Box 1, Stoneman Papers) or in Robin Kincaid's recollections (interview, February 18, 1985, in unnumbered box, Whitman Bassow Papers, Library of Congress). Lyons's later recollections are quoted from Crowl, *Angels in Stalin's Paradise*, 161, citing letters from Lyons (June 20, 1977) and from Armand Paul Ginsberg for Lyons (July 2, 1977). Duranty biographer S. J. Taylor shares some of my doubts: *Stalin's Apologist*, 207, 235–36.

conditions are bad, but there is no famine.”⁴⁵ While the criticisms of Jones included new concepts, Duranty’s basic formula (shortages, even malnutrition, but no famine) carried over from his November series.

Fischer, then touring the United States, needed little official encouragement to rail against famine reports. He spent the spring of 1933 campaigning for American diplomatic recognition of the USSR. As rumors of a famine there reached American shores, Fischer vociferously denied the reports. He agreed that Russians were “hungry—desperately hungry” but attributed this to Russia’s “turning over from agriculturalism to industrialism.” In each city he visited, Fischer flatly denied that mass starvation existed in Russia. Arguing that there were shortages but no famine, Fischer declared in another speech that the Russian peasant would endure such hardships “as long as the fulfillment of his objective is visible to the naked eye, in the form of industrial achievement.” Upon his return to Russia later that summer, Fischer’s story changed only slightly. His first article from Moscow, entitled “Russia’s Last Hard Year,” stated, “The first half of 1933 was very difficult indeed. Many people simply did not have sufficient nourishment.” Fischer blamed poor weather and the refusal of peasants to harvest the grain, which then rotted in the fields. Government requisitions drained the countryside of food, he admitted, but military needs (a potential conflict with Japan) explained the need for such deadly thoroughness in grain collections.⁴⁶

While Fischer used the threat of war as a justification for hardships, Duranty continued to employ war in a metaphorical sense. Perhaps inspired by the Soviets’ own rhetoric, he continued to compare collectivization to a battle between the modernizing Bolsheviks and backward peasants. Off on another vacation in April—this time to Greece—Duranty organized the trip so that he could travel through Ukraine. Gazing out the windows and speaking with peasants at the stations along the way, Duranty concluded that the rumors about a famine were unsubstantiated—always attributed to the next village. Duranty still maintained his optimism for the future: “an end has been made of the muddle and mismanagement of the past two years, and . . . Moscow is taking an interest” in the peasants.⁴⁷

By late spring, Gareth Jones rebutted Duranty in a stinging counterattack. Jones reiterated his assessment of famine conditions, claiming it was based on conversations with numerous foreign diplomats in addition to peasants in more than twenty villages. He also cited Muggeridge’s late March series in the *Manchester Guardian* as corroboration. Lashing out against the Moscow-based journalists, Jones called them “masters of euphemism and understatement,” thanks to ever-stricter censor-

⁴⁵ Walter Duranty, “Russians Hungry, but Not Starving,” *NYT*, March 31, 1933. He used the phrase earlier, in a poetic effort: Duranty, “Red Square,” *NYT Magazine*, September 18, 1932.

⁴⁶ Fischer, *Men and Politics*, 206–09; reports on Fischer’s lectures appear in “‘New Deal’ Needed for Entire World, Says Visiting Author,” *Denver Post*, April 1, 1933, cited in Cowl, *Angels in Stalin’s Paradise*, 157; “Too Much Freedom Given to Russia’s Women, Says Writer,” *San Francisco News*, April 11, 1933; and “New Economic Society Coming out of Russia,” *Milwaukee Leader*, March 14, 1933, both in Box 60, Louis Fischer Papers, Mudd Library, Princeton University; Fischer, “Russia’s Last Hard Year,” *Nation* 137 (August 9, 1933): 154.

⁴⁷ Duranty, *Duranty Reports Russia*, 313 (April 6, 1933); Walter Duranty, “Soviet Peasants Are More Helpful,” *NYT*, May 14, 1933 (dateline Odessa, by mail to Paris, April 26, 1933). On the trip routing, see Duranty to James, n.d. [mid-April 1933?]; and James to Duranty, April 21, 1933, both on reel 32, James Papers; Duranty, *I Write as I Please* (New York, 1935), 61.

ship. The letter closed on a bitter congratulatory note: the Soviets' combination of food distribution policy (so that Moscow remained "well-fed") and censorship had managed to "hide the real Russia."⁴⁸

By June, Duranty pleaded to travel abroad again. Until the midsummer harvest, he told his editor, things in Moscow would be "dull." The *New York Times* editors scotched the trip, so Duranty redirected his complaints to a friend and fellow journalist. As for food supplies, he wrote his friend: "The 'famine' is mostly bunk as I told you except maybe Kazakhstan and the Altai where they wouldn't let you go . . . The [NKID] in particular is rather crotchety about reporters travelling these days." Stuck in Moscow, bored, Duranty returned to one of his favorite themes, Russian suffering. He referred to Bolsheviks as "fanatics [who] do not care about the costs in blood or money." Suffering in Russia, he stressed, was not strictly a Soviet phenomenon: "It is cruel . . . but the Union of Soviet Socialist Republics is near to cruel Asia, and the proverb 'One Life, One Kopeck' was a century-old expression of human values in Czarist Russia." The article closed with the acknowledgment that "life here is hard and menaced by malnutrition and diseases that arise therefrom," but it once again underlined the ultimate goal justifying these sacrifices: the leadership's "fanatic fervor" for industrialization.⁴⁹

An early August dispatch dealt once more with rumors about the famine. Duranty attributed them to the anti-Bolshevik émigré "rumor factories" in neighboring states. Soviet authorities, Duranty wrote, had inadvertently abetted these factories by adopting an "ostrich policy . . . in trying to hide it [the food shortage] and some of its consequences." Such shortages had taken "a heavy toll of Soviet fortitude and even Soviet lives," reducing the food supply "below what are generally regarded as the minimum requirements." Shortly thereafter, Duranty cabled his editor that, in spite of these persistent rumors, the word "famine" should be avoided in news coverage—in favor of the above formulation about minimum requirements. He maintained his silence about the travel ban. When the *New York Times* published an article about restrictions on travel, it came from the Associated Press rather than the newspaper's own correspondent. Below the AP story, though, Duranty wrote an article, "Famine Report Scorned," praising the new harvest without denying past problems: "Until this harvest the picture was dark enough. The Kremlin had ruthlessly carried through the agrarian revolution of collective farming, and the costs had been heavy for the Russian people, but it looks now as if the revolution is complete because the harvest is really good."⁵⁰

Duranty seemed genuinely confused about the continuation of the travel ban, given the improved conditions. His letter to the foreign editor complained: "The poor goofs [in the NKID Press Office] have chosen this moment, when the harvest

⁴⁸ "Mr. Jones Replies" [letter to the editor], *NYT*, May 13, 1933.

⁴⁹ Duranty to *New York Times*, June 17, 1933, and James to Arthur Sulzberger, June 17, 1933, both on reel 33, James Papers. Duranty to H. R. Knickerbocker, June 27, 1933, catalogued correspondence, H. R. Knickerbocker Papers, Columbia University Library. Walter Duranty, "Russian Suffering Justified by Reds," *NYT*, July 9, 1933. "One Life, One Kopeck"—the title of Duranty's first novel—is a translation of the phrase *zhizn' kopeika*.

⁵⁰ Walter Duranty, "Russian Emigres Push Fight on Reds," *NYT*, August 12, 1933. Duranty to James, August 19, 20, and 22, 1933, James to Duranty, August 22, 1933, all on reel 33, James Papers. "Moscow Doubles Price of Bread" [AP], *NYT*, August 21, 1933; Duranty, "Famine Report Scorned," *NYT*, August 27, 1933.

REALLY IS GOOD, to forbid foreign correspondents to travel.” He once again denied that there was a famine per se, “but there was a heavy loss of life and much suffering and now of course is the moment to see and say that things are better. But the [NKID] doesn’t seem to understand that.” Ignoring Duranty’s repeated injunctions against the word “famine,” the *New York Times* editors printed articles from Vienna and Berlin that used the word. They even printed one of Duranty’s own articles under the headline “Famine Toll Heavy in South Russia.” That article continued his usual themes: there was loss of life, not from starvation but from diseases “due to lower resistance”; the death rate was three times higher than normal in Ukraine, North Caucasus, and the Lower Volga—but no “famine” existed.⁵¹

Poor conditions in the USSR seemed to affect Duranty’s mood if not his reporting. He complained to his editors that he had tired of working overseas, especially in Moscow. Duranty distinguished himself from those whose bright future justified present hardships: the USSR “may someday be a paradise for a future generation of Russians, but I am not a future generation, nor, thank God, a Russian.” He proposed working only part-time in Moscow, writing primarily feature articles. The *Times* senior editors, all dissatisfied with Duranty’s frequent absences from Moscow, were happy to accept this arrangement.⁵²

The NKID gave Duranty one last scoop before he stepped down. The Press Office informed him in late August that he could travel through the Ukrainian countryside. Permission to travel, however, did not imply the right to travel freely. The restrictions on his itinerary perplexed Duranty. According to one British diplomat, “Mr. Duranty professed to be much irritated by this action, which he felt had cut the ground from under his feet by obliging him to recognize a ban upon his movements.”⁵³

Yet Duranty did not mention the restrictions to his editors. He instead boasted that he and AP correspondent Stanley Richardson soon would be taking a trip to Ukraine and the North Caucasus to challenge the “campaign about the alleged famine.” The editors received this news enthusiastically, urging him to leave as soon as possible. This trip earned these two reporters no little resentment in the foreign colony—a problem for the NKID as well as for Duranty. As the foreign-policy chief wrote to the head of the secret police:

After foreign correspondents Duranty and Richardson set out, with our permission and your agreement, on their trip to Ukraine, many other foreign correspondents asked for

⁵¹ Duranty to Frederick Birchall, August 23, 1933, reel 63, James Papers. “Cardinal Asks Aid in Russian Famine,” *NYT*, August 20, 1933; Birchall, “Famine in Russia Held Equal of 1921,” *NYT*, August 25, 1933.

⁵² On Duranty’s dissatisfaction, see Duranty to James, August 15, 1933, Adolph Ochs Papers, *New York Times* Archive; Duranty to Birchall, August 15, 1933, reel 63, James Papers; and Whitman Bassow, *The Moscow Correspondents: Reporting Russia from the Revolution to Glasnost* (New York, 1988), 88. His editors’ complaints are contained in James to Sulzberger, August 2, 1933, Birchall to James, August 16, 1933, both in personnel files, Arthur Hays Sulzberger Papers, *New York Times* Archive; James to Sulzberger, August 23, 1933, reel 63, James Papers; James to Adolph Ochs, September 5, 1933, Ochs Papers. The NKID Press Office was well aware of these tensions: see Podol’skii diary, December 31, 1933, AVPRF, f. 0129, op. 15, pap. 128a, d. 335, l. 16.

⁵³ Edward Coote to Sir John Simon, September 12, 1933, in Carynnyk, *Foreign Office and the Famine*, 307.

permission for trips to the south . . . Since I cannot be up-to-date [*v kurse*] on conditions in the various regions to which the foreign correspondents would like to go, I am asking you to give us your conclusions after weighing all of the circumstances. Personally, it seems to me that the moment has come when we can be more liberal on the issue of foreign correspondents' movements, that is, on the extremely irritating strict application of the rules about their trips outside Moscow.⁵⁴

Once under way, Duranty and Richardson traveled first to Rostov and then Kharkov. Duranty's reports contained the same set of contradictions as his series on food shortages from the previous November. The first report began by asserting, "The use of the word 'famine' in connection with the North Caucasus is a sheer absurdity." After gloating that "even a child can see that this is not famine but abundance," Duranty revised downward his earlier estimate that mortality had tripled. Upon reaching Ukraine, Duranty's evaluation was far more bleak, resorting again to his wartime analogies: the Kremlin "has won the battle with the peasants," although "the cost has been heavy." The whole episode could be summed up briefly, Duranty wrote: "Hunger had broken [Ukrainians'] passive resistance—there in one phrase is the grim story of the Ukrainian Verdun." Here, Duranty wrote more explicitly about the costs: "hard conditions . . . had decimated the peasantry."⁵⁵

In his private conversations, Duranty described the famine's results more graphically. In an oft-cited incident reported by Eugene Lyons, Duranty apparently stopped by Lyons's apartment upon returning from his travels. Lyons recalled,

He gave us his fresh impressions in brutally frank terms and they added up to a picture of ghastly horror. His estimate of the dead from the famine was the most startling I had as yet heard from anyone.

"But, Walter, you don't mean that literally?" Mrs. McCormick exclaimed.

"Hell I don't—I'm being conservative," he replied, and as if by way of consolation he added his famous truism: "But they're only Russians."

While Lyons did not repeat Duranty's mortality figure in this 1937 recollection, other sources suggest that, upon returning from Ukraine, Duranty estimated that between 7 and 10 million had died "directly or indirectly from lack of food."⁵⁶

William Henry Chamberlin also petitioned to travel into the famine areas in late August, but the NKID Press Office denied his initial request. The *Christian Science Monitor* printed an Associated Press story about this denial, referring to its desire to report on the impact of the food shortage "last winter."⁵⁷ Shortly afterward, Chamberlin wrote a casual letter to a friend that explained the travel ban as related

⁵⁴ Duranty to James, August 28, 1933, James to Duranty, August 29, 1933, Birchall to James, August 31, 1933, all on reel 33, James Papers. Litvinov to Iagoda, September 13, 1933, AVPRF, f. 05, op. 13, pap. 90, d. 14, l. 73.

⁵⁵ Walter Duranty, "Soviet Is Winning Faith of Peasants," *NYT*, September 11, 1933; Duranty, "Abundance Found in North Caucasus," *NYT*, September 16, 1933; Duranty, "Big Soviet Crop Follows Famine," *NYT*, September 16, 1933; Duranty, "Soviet's Progress Marked in a Year," *NYT*, September 21, 1933.

⁵⁶ Lyons, *Assignment in Utopia*, 579–80. "Mrs. McCormick" refers to distinguished *New York Times* foreign correspondent Anne O'Hare McCormick, then visiting the Lyonses. A similar story appears in Malcolm Muggeridge, *Chronicles of Wasted Time* (London, 1972), 1: 254–55; Strang to Simon, September 26, 1933, in Carynnyk, *Foreign Office and the Famine*, 310–13.

⁵⁷ Chamberlin, *Confessions of an Individualist*, 154–55; "Soviet Restricts Alien Reports as Food Wanes," *Christian Science Monitor*, August 21, 1933.

to “what has happened rather than . . . what is happening now” in the countryside. He went on in an optimistic tone, predicting that “this year’s crop . . . is exceptionally good, and, while there are familiar difficulties in harvesting and transporting it, the signs seem to point to an easier winter. Everything in this world is, of course, highly relative.” At the same time, Chamberlin also submitted a signed opinion piece to the *Monitor*, part of an occasional series called “Diary of an Onlooker.” Chamberlin reported on contradictory rumors floating around Moscow about the situation in the Soviet countryside. Based on a report from “a foreign agricultural expert with a knowledge of the Russian language and long experience in various parts of the country” (perhaps his friend Otto Schiller?), Chamberlin announced that events in Russia gave “some measure of confirmation to both the optimistic and the pessimistic reports.” This unnamed expert “confirmed the prevalent stories of widespread acute distress and hunger in the southern and southeastern parts of the country.” Still, Chamberlin optimistically insisted that “there would be some increase in the agricultural production, measured by the extremely low level it touched last year.” Better weather fueled Chamberlin’s hope for improvement, as did the “fear of hunger” and the effectiveness of new repressive machinery. The section closed with the observation that Muscovites were choosing vacation spots far from Ukraine, in part because of the reports of poor conditions there.⁵⁸

After the Duranty/Richardson trip, Chamberlin finally received permission to travel with his wife through the afflicted regions in late September. Journalist William Stoneman sent the first word back to the States about their travels: “Chamberlin says after a two week trip . . . that 30% of the people in some villages died of typhus & famine. It must have been a ghastly spring in the villages.” Stoneman did report one note of optimism, though: central authorities “have plenty [of grain] to support the cities, to replenish the army stores and to give more to the villages.” Shortly after returning to Moscow, Chamberlin visited his friend William Strang in the British Embassy. According to Strang, Chamberlin “often asked himself why the population did not flee *en masse* from the famine areas. He could only attribute their immobility to the characteristic Russian passivity of temperament. In the Ukraine he had the impression that the population could find nothing better to do than die as a protest.”⁵⁹ Chamberlin thus explained the course (if not the cause) of the famine in terms of peasant passivity.

While the *Monitor* did not print Chamberlin’s reports from Ukraine, the *Manchester Guardian* ran them as a five-part series under the rubric “The Soviet Countryside: A Tour of Inquiry.” The early articles referred to “famine” conditions, and actions that were “no less ruthless than those of war,” but also noted the “excellent crop” for 1933 and closed with a familiar statement about Russia as a

⁵⁸ Chamberlin to Calvin Hoover, September 25, 1933, Addition to Calvin Hoover Papers, Duke University Archives; William H. Chamberlin, “Diary of an Onlooker in Moscow,” *Christian Science Monitor*, August 17, 1933.

⁵⁹ Stoneman to Samuel Harper, October 12, 1933, Box 18, Samuel Northrop Harper Papers, University of Chicago; Strang to Simon, October 14, 1933, in Carynnyk, *Foreign Office and the Famine*, 334. The Chamberlin-Strang friendship (mentioned in a 1968 interview) is reported in Myers, “William Henry Chamberlin,” 54–55—though Strang makes no mention of Chamberlin in his memoir, William Strang, *Home and Abroad* (London, 1956).

“land of paradoxes.” In the final article, Chamberlin mused about peasant inaction, searching for a “psychological explanation of this curious fatalism.” He concluded that “those who died were . . . old-fashioned peasants who simply could not conceive of life without their individual farm.” Even though Chamberlin discussed famine conditions openly, his reporting placed ample blame on the peasants’ conservatism and recalcitrance.⁶⁰

Reports filed by Duranty and Chamberlin in the autumn of 1933 sounded quite similar. The *New York Times* reporter, for instance, tallied the results of the Five-Year Plan in an article entitled “Russia’s Ledger.” The costs of industrialization had been “prodigious, not only in lowered standard of living but in human suffering, even in human lives.” Yet Duranty did not blame Soviet policy; the fault lay instead with the “innate conservatism of the farmer.” Political liberties had been trampled by the “attempt of the Bolsheviks to submerge the individual in the state”—but such should be expected of Russia’s political tradition, which so closely resembled the “despotism of Asia.” Russian character—in this instance, at both individual and societal levels—explained Russian conditions. Duranty did not dwell on his recent trip, but he did assess Russian suffering as Chamberlin had: the previous year had “tightened the belts of the Russian people to an almost, but not quite, intolerable degree.”⁶¹ In reports based on their respective trips through the famine regions, Duranty and Chamberlin both emphasized the human costs. Both remained optimistic that the worst had passed. And, most strikingly, both blamed peasants’ hardships on their own passivity as much as on Soviet policy.

IN THE LONG RUN, their travels led Duranty and Chamberlin toward sharply divergent views of the Soviet Union. Chamberlin’s trip into the countryside marked the most important event in his once-gradual estrangement from the Soviets. While most of his reports filed before the trip—and even immediately afterward—shared much with Duranty’s and Fischer’s, Chamberlin subsequently altered his view of collectivization as a result of these travels. Whereas Chamberlin had earlier considered peasant “backwardness” an impediment to collectivization, he later came to believe the opposite, as evidenced by this observation: “It was not the more backward peasants, but the more progressive and well-to-do, who usually showed the greatest resistance to collectivization, and this not because they did not understand what the new policy would portend, but because they understood too well.”⁶² This view, appearing in his articles and books published in 1934, amounted to a recantation of his earlier ideas.

⁶⁰ All *Manchester Guardian*: “Second Agrarian Revolution,” October 17, 1933; “Some Cossack Villages,” October 18, 1933; “Ukrainian District’s Good Harvest,” October 19, 1933; “New Russian Agriculture—Two Main Types,” October 20, 1933; “Villages around Kiev—Final Impressions,” October 21, 1933.

⁶¹ Walter Duranty, “Russia’s Ledger: Gain and Cost,” *Duranty Reports Russia*, 329–41 (October 1, 1933).

⁶² Chamberlin discussed the famine (quoted above) in William Henry Chamberlin, *Russia’s Iron Age* (Boston, 1934), 76–77. Recollections that place the famine as a central event in Chamberlin’s Russian career include Chamberlin, “My Russian Education,” in *We Cover the World by Sixteen Foreign Correspondents* (New York, 1937), 238; Chamberlin, *Confessions of an Individualist*, 143; Chamberlin, *Evolution of a Conservative* (Chicago, 1959), 11. Thanks to D’Ann Penner for stressing the nature of Chamberlin’s later views.

Цвага! УКРАЇНЬСЬКА ТРУДЯЩА ГРОМАДА Цвага!

Заходом відділів Союзу Українських Робітничих Організацій

— відбудеться —

МАСОВЕ ВІЧЕ

в Четвер 28-го Грудня 1933 р.

Початок о год. 7:30 вечером

в Між. Роб. Домі

3014 Yemans Ave. Hamtramck, Mich.

На вічу будуть обговорені і вияснені слідуючі питання:

- 1.—Причини визнання Союзу Радянських Соціалістичних Республік Америкою і кампанія Білогвардійщини російської і української проти визнання.
- 2.—Чому українські націоналісти не помагають потерпівшим від повіді голодуючим селянам на західні Україні, а пропагують голод і людодіцтво на Радянській Україні?
- 3.—Причини голоду і злиднів селян на Західні Україні і наша допомога для них.

На віче кличемо всіх що співчують визвольні боротьбі українських трудящих, за їх соціальне освободження з лабет польського, румунського і чехословацького капіталізму. На вічу будуть промовляти місцеві і позамісцеві бесідники.

ВСТУП ВІЛЬНИЙ. ВІЧЕВИЙ КОМІТЕТ.

ВИКЛИК НА ДИБАТУ

Окружний Комітет Союзу УРО апелює до членів українських заходових організацій як Укр. Народного Союзу і Укр. Роб. Союзу: Члени вище згаданих організацій, домагайтесь від своїх редакторів «Свободи» і «Народної Волі» щоб вони виступили публічно в Детройті на дебат. Запевняємо вас що наші редактори з Укр. Щоденних Вістей, на наше домагання можуть явитися. Тягніть Ви своїх! Нехай публічно з трибуни доказують про дійсний стан положення і голод як на Радянській так і на Західній Україні. За дальшими інформаціями заінтересовані справою зголошуйтеся на адресу окружного секретаря:

Окружний Секретар Союзу У.Р.О. 4959 Martin Ave. Detroit

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The Ukrainian Workingmen's Association announced a meeting it was convening in December 1933 to discuss the famine in relation to the commencement of diplomatic relations between the Soviet Union and the United States. From the vertical files in the Reference Library of the Harvard Ukrainian Research Institute, reproduced here with permission.

But in other articles appearing in the months after his harrowing trip through the devastated countryside, Chamberlin still expressed ambivalence about collectivization. After detailing, in one widely circulated article, the destruction wrought by

famine, Chamberlin sounded a note of optimism: the “tenacious vitality [of] the semi-Asiatic peasantry” ensured that “recovery comes more easily than might be the case in a softer country.” National character remained a crucial factor in Chamberlin’s explanations of Soviet events, even as his political position began to shift. Reviewing a book of Duranty’s collected dispatches, furthermore, Chamberlin defended the legitimacy of the *New York Times* reporter’s claims: “Duranty consistently takes the line, a perfectly logical and defensible one, that the sufferings which, as he recognizes, have been and are being imposed on the Russian people in the name of socialism, industrialization, and collectivization are of small account by comparison with the bigness of the objectives at which the Soviet leaders are aiming.” Similarly, in one 1934 article containing his estimate of 4 million famine-related deaths, Chamberlin repeated his earlier argument that “the poor harvest of 1932 was attributable in some degree to the apathy and discouragement of the peasants.”⁶³

Unlike Chamberlin or Duranty, Fischer did not write, either publicly or privately, about living conditions through the remainder of 1933. A November letter to a friend promised only that he would give him the “lowdown” when they next met. Fischer’s first mention of “the Ukrainian famine of 1933”—in a 1934 article from and about Spain—connected the famine to “prodigious efforts, now already crowned with considerable success, to give the country a new and permanently healthy agrarian base.” Fischer did not directly address the “difficulties” of 1933 until well after the fact—in a 1934 *Nation* article, “In Russia Life Grows Easier.” Those articles focused on Russia’s “bright prospects,” and improved supplies of clothing and food in major Soviet cities. These economic improvements had led to a decline in political opposition, which Fischer hoped would lead, in turn, to a curtailment of secret-police activities.⁶⁴

Fischer maintained his general optimism about the Soviet Union through the publication of his *Soviet Journey* in 1935. The book devoted three pages to a discussion of the famine of 1932–1933, in which Fischer described his October travels through Ukraine. He told of food left rotting in the fields as the result of peasants’ “passive resistance.” Fischer blamed the peasants directly for having “brought the calamity upon themselves,” and History itself provided the explanation:

It was a terrible lesson at a terrific cost. History can be cruel. The Bolsheviks were carrying out a major policy on which the strength and character of their regime depended. The peasants were reacting as normal human beings would. Let no one minimize the sadness of

⁶³ William Henry Chamberlin, “Ordeal of the Russian Peasantry,” *Foreign Affairs* 12 (April 1934): 503, 505; Chamberlin, “The Balance Sheet of the Five-Year Plan,” *Foreign Affairs* 11 (April 1933): 458, 466; Chamberlin, “As One Foreign Correspondent to Another,” *Christian Science Monitor Magazine*, May 2, 1934. While many critics of Duranty and Fischer have cited the chapter in Chamberlin’s *Russia’s Iron Age* entitled “The Ordeal of the Russian Peasantry,” fewer have cited his article with the same title in *Foreign Affairs*. Although the materials appear to have been written within a month of each other—and many paragraphs appear in both pieces—they differ substantially in tone. The stand-alone article focuses on character traits such as apathy and tenacity far more than the book does. One intermediate argument connects peasant apathy to the economic and extra-economic measures of the Soviet state; see Chamberlin, “Russia without the Benefit of a Censor: Famine Proves Strong Weapon in Soviet Policy,” *Christian Science Monitor*, May 29, 1934.

⁶⁴ Fischer to Alexander Gumberg, November 5, 1933, folder 2, Box 7, Alexander Gumberg Papers, State Historical Society of Wisconsin, Madison. Louis Fischer, “Class War in Spain,” *Nation* 138 (April 18, 1934): 437; Fischer, “In Russia Life Grows Easier,” *Nation* 138 (June 13, 1934): 667, 668; Fischer, “Moscow Reports Progress,” *Fortnightly Review*, n.s., 135 (June 1934): 651–57.

the phenomenon. But from the larger point of view the effect was the final entrenchment of collectivization. The peasantry will never again undertake passive resistance.⁶⁵

Like Duranty and Chamberlin, Fischer stressed the positive results ensuing from Bolshevik victory in the countryside and connected the famine to peasant action (or inaction).

The issues resurfaced in 1935, when Chamberlin and Fischer traded blows over their reporting in 1933. After a week-long series on a famine raging in the USSR appeared in Hearst newspapers, Fischer published a rebuttal of these claims in *The Nation*.⁶⁶ Fischer, Lyons, and Chamberlin all agreed that there was no famine in Russia in 1935; Lyons, for one, called the Hearst series “patently doctored.” But Chamberlin used the occasion to blast Fischer, sarcastically arguing that Fischer’s denial of a 1935 famine made no mention of the famine of 1932–1933, which affected (in Chamberlin’s words) “Ukraina [sic], the North Caucasus, considerable districts of the Lower and Middle Volga, and Turkestan.” Claiming that Fischer had yet to make any “single, forthright unequivocal recognition of the famine,” Chamberlin accused Fischer of using “misleading euphemistic terms” to describe Soviet events. Fischer’s reply to Chamberlin, published in the same issue, defended his treatment of the famine and then turned the tables, accusing Chamberlin of one-sidedness for blaming only the Soviet government. If the famine was “man-made,” as Chamberlin had charged, then “the peasants were the men who made it,” wrote Fischer.⁶⁷

By the time of this dispute over the famine hoax, the four protagonists had parted ways. While all had started the decade positively inclined (to greater or lesser degrees) toward the Soviets, Lyons and Chamberlin had grown disenchanted with and even disgusted by the “Soviet experiment” by 1935. After the mid-1930s, these latter writers began writing slashing criticisms of both the Soviet Union and its American supporters. Duranty and Fischer quickly became targets, especially for their writings on the famine of 1932–1933. Fischer eventually did reconsider his views about the Soviet Union, writing about his new perspective with more thoughtfulness and considerably less acid than Lyons and Chamberlin had. Fischer’s essay in the widely read *The God That Failed* (1949) attributed the famine to “Bolshevik haste and dogmatism.” Reflecting on his fifteen years of enthusiastic support for the USSR, Fischer concluded that he had been “glorifying steel and kilowatts and forgetting the human being.”⁶⁸ Duranty, unlike the others, never recanted his earlier views outright. Later writings mentioned the famine, calling it

⁶⁵ Louis Fischer, *Soviet Journey* (New York, 1935), 174, 108, 170–72 (on famine). The trip through Ukraine is described in Fischer, “Soviet Progress and Poverty,” *Nation* 135 (December 7, 1932): 552–55.

⁶⁶ The articles appeared under the byline “Thomas Walker” in the *New York Evening Journal*, February 18, 19, 21, 25, and 27, 1935, as cited in Dalrymple, “Soviet Famine of 1932–1934,” 256 n. 46. Louis Fischer, “Hearst’s Russian ‘Famine,’” *Nation* 140 (March 13, 1935): 296–97.

⁶⁷ William Henry Chamberlin, “The Ukrainian Famine” [letter to the editor], *Nation* 140 (May 29, 1935): 629; Fischer, “Louis Fischer’s Interpretation” [reply], *ibid.*, 629–30; Lyons, *Red Decade*, 141. See also Freda Kirchwey’s letters to Fischer, March 14 and 22, 1935, and June 1935, folder 168, Box 10, Freda Kirchwey Papers, Schlesinger Library, Radcliffe College. This last letter noted the extensive controversy about the Chamberlin-Fischer exchange and celebrated the resulting increase in newsstand sales.

⁶⁸ Louis Fischer, untitled essay in *The God That Failed: Six Studies in Communism*, Richard Crossman, ed. (1949; rpt. edn., New York, 1959), 188–89.

“man-made” but wavering as to its origins. By 1949, Duranty’s final book offered an apology of sorts: “Whatever Stalin’s apologists may say, 1932 was a year of famine.” While it may have appeared unintentional to those “on the spot,” he explained, he now believed that authorities should be blamed for their actions. The section closed by quoting Stalin: “Why blame the peasant? . . . For *we* [the Communist Party] are at the helm.”⁶⁹

Lyons’s and Chamberlin’s rancor covered up their own actions and writings during the famine year—some of which bore marked similarities to those of their targets, Duranty and Fischer. In 1932–1933, all four authors portrayed the battle between the party and the countryside as one between determined modernizers and recalcitrant, fatalistic peasants. While reporting—and regretting—the loss of peasant lives, all four authors framed the loss of life as a necessary cost in the struggle for economic progress. All four journalists, furthermore, deployed stereotypes about Russian peasants in order to explain peasant actions (or ostensible inaction). Fischer and Chamberlin explicitly linked the horrible fate of the Soviet peasantry to visions of a modern, industrial society. The expression, repeated by these two as well as other journalists and scholars, that the Five-Year Plan represented Russia’s attempt to “starve itself great” emphasized the hoped-for ends of industrialization over the brutal means.⁷⁰

Enthusiasm for Soviet economic development led American Russia-watchers of all political persuasions to support or at least withhold judgment on Soviet Five-Year Plans. This “romance of economic development” explains the widespread American support for the USSR far better than Lyons’s harangues about “the Stalinist penetration of America.”⁷¹ Many commentators approved of Soviet-style industrialization while denouncing communism. Their support for Soviet efforts to modernize a “backward” nation came in spite of their recognition of the tremendous human costs entailed. Even though they had some information about rural conditions during the famine, American observers had an easier time finding the sacrifices worthy because they considered the people sacrificed so unworthy. Common stereotypes about Russians served to explain their struggles and suffering. Conservative and apathetic peasants could be trusted to resist (but only passively) Soviet plans. To bring about important changes, so the logic went, would entail extreme hardships and even significant loss of life—which the peasants, fatalistic and inured to suffering, were especially well suited to endure. National-character

⁶⁹ Walter Duranty, *Stalin and Co.: The Politburo—The Men Who Run Russia* (London, 1949), 68–69; Taylor, *Stalin’s Apologist*, 236–37. Duranty is loosely translating Stalin’s speech of January 11, 1933, “O rabote v derevne,” *Sochineniia*, 13 vols. (Moscow, 1952), 13: 233, italics in original.

⁷⁰ Fischer is quoted in *Experiences in Russia—1931: A Diary* (Pittsburgh, 1931), 85. Other instances include H. R. Knickerbocker (a journalist and close friend of Duranty’s), “Everyday Russia,” in *The New Russia: Eight Talks Broadcast by the BBC* (London, 1931), 21; Bruce Hopper to Hamilton Fish Armstrong, January 18, 1930, Box 35, Armstrong Papers; and Boris Brutzkus, *Economic Planning in Soviet Russia* (London, 1935), 226.

⁷¹ See, for instance, Richard H. Pells, *Radical Visions and American Dreams: Culture and Social Thought in the Depression Years* (New York, 1973); Paul Hollander, *Political Pilgrims: Travels of Western Intellectuals to the Soviet Union, China and Cuba* (1984; rpt. edn., Lanham, Md., 1991), chap. 3; Frank A. Warren, *Liberals and Communism: The “Red Decade” Reconsidered* (New York, 1966); John P. Diggins, “Limping after Reality: American Intellectuals, the Six Myths of the USSR, and the Precursors of Anti-Stalinism,” in *Il mito dell’URSS: La cultura occidentale e l’Unione Sovietica*, Marcello Flores, ed. (Milan, 1990); and Eduard Mark, “October or Thermidor? Interpretations of Stalinism and the Perception of Soviet Foreign Policy in the United States, 1927–1947,” *AHR* 94 (October 1989): 937–62.

stereotypes thus combined with enthusiasm for economic development to resolve the tensions between ends and means in American writings on the USSR. As anticommunist economist Calvin Hoover put it, Russian peasants would not rise from their "Asiatic" laziness unless prompted by the "immediate stimulus of hunger."⁷² The worthy goal of modernization, Hoover and others implied, could be reached only through difficult if not violent means.

This dilemma of ends and means persisted through Soviet and post-Soviet Russia. Writers in Mikhail Gorbachev's Russia (and other parts of the collapsing Soviet Union) wrestled with the historical meaning of the tragedies of the 1930s. A plaintive assessment by two journalists in 1990 stands out in this often nasty debate. Concluding a newspaper article on new research on the famine of 1932–1933, these writers struggled to sum up the Soviet period in a single paragraph: "It is not true," they wrote, "that nothing good was created [under the Soviets]. It is true that everything good came at too high a price."⁷³

Most Western journalists in Stalin's Moscow, spared the high price paid by the Russians, reached less poignant conclusions. Chamberlin noted the great loss of life but placed it in the context of Soviet goals: the villages he visited in the famine's aftermath, he wrote at the time, stood as "grim symbols of progress." Duranty, for his part, insisted that the peasants who died in the battle for control of the countryside had become "victims on the march toward progress."⁷⁴ That the march was a forced one, prodded by Soviet bayonets, concerned these journalists less than the ostensible destination.

THE SOVIET PATTERN OF THE EARLY 1930s—a devastating famine, the very existence of which was contested abroad—reappeared with alarming precision during China's Great Leap Forward (1958–1960).⁷⁵ The all-out Chinese attempt at collectivization (like the Soviet case, designed to funnel resources to the industrial sector) led to chaos in the Chinese countryside. Government authorities instituted collectivization and political repression to gather whatever food they could to further economic and political goals. The breadbaskets of China, stripped of all food, became home to mass starvation, with death toll estimates as high as 26 million.⁷⁶

⁷² Calvin B. Hoover, *Economic Life in Soviet Russia* (New York, 1931), 85.

⁷³ S. and P. P. Zavorotnyi, "Operatsiia Golod: Vosem' mesiatsev 1932–33 goda unesla milliony krest'ianskikh zhiznei," *Komsomol'skaia pravda*, February 3, 1990.

⁷⁴ William Henry Chamberlin, "Some Cossack Villages," *Manchester Guardian*, October 18, 1933; Duranty, *I Write as I Please*, 288.

⁷⁵ While finding many commonalities in the Soviet and Chinese famines, Thomas P. Bernstein also notes differences, most notably that Soviet authorities (unlike the Chinese twenty-five years later) saw the peasants as the enemy of the state; see Bernstein, "Stalinism, Famine and Chinese Peasants: Grain Procurements during the Great Leap Forward," *Theory and Society* 13 (May 1984): 339–78. I am also indebted to D'Ann Penner's published ("Agrarian 'Strike'") and unpublished work for comparisons of the Soviet and Chinese famines. General background on the origins and operations of the Great Leap Forward can be gleaned from Roderick MacFarquhar, *The Origins of the Cultural Revolution*, Vol. 2, *The Great Leap Forward, 1958–60* (New York, 1983), esp. chaps. 5, 8; and David Bachman, *Bureaucracy, Economy, and Leadership in China: The Institutional Origins of the Great Leap Forward* (Cambridge, 1991).

⁷⁶ Jasper Becker, *Hungry Ghosts: Mao's Secret Famine* (New York, 1996), chap. 18. More careful analyses are found in Penny Kane, *Famine in China, 1958–61: Demographic and Social Implications* (New York, 1988), 84–90.

Western observers denied the existence of the famine in terms strikingly similar to those used by Moscow-based reporters in the 1930s. Edgar Snow, whose *Red Star over China* (1938) introduced Mao Zedong to the English-speaking world, returned to China in 1960. Rumors of famine clearly weighed on Snow's mind, but he denied them outright: "I saw no starving people in China, nothing that looked like old-time famine." His travelogue later repeated the line of argument used by Duranty, Fischer, and Ralph Barnes regarding the Soviet famine: "Considerable malnutrition undoubtedly existed. Mass starvation? No."⁷⁷ Snow's earlier writings on Chinese famine, based on his travels through northwest China in 1929–1931, furthermore, shared much with the Moscow correspondents. In that analysis, he complained that residents of the famine region did not take any steps to prevent or even delay their deaths: "I was profoundly puzzled by their passivity. For a while I thought nothing would make a Chinese fight."⁷⁸ Peasant inaction, as much as government action, had been a central factor in Snow's Chinese famines.

While Snow's case is the most famous because he was granted permission to travel through the famine regions, other Western specialists came to the same conclusions without firsthand experience. Like those Western experts who stressed the global significance of the "Soviet experiment" in the 1930s, some China specialists of the 1960s trumpeted the achievements rather than the costs of the Great Leap Forward.⁷⁹ The Hong Kong-based *Far Eastern Economic Review*, for instance, editorialized that "what is happening in China is of momentous importance . . . [as] a new model for human society and a new method of overcoming poverty."⁸⁰ Scholars such as Gunnar Myrdal and John King Fairbank, the dean of American Sinology, downplayed or dismissed rumors of famine conditions.⁸¹

Debates about foreign coverage of the Chinese famine have resurfaced in recent years, with increasing acrimoniousness. Some Western Sinologists who had once been more sympathetic to the People's Republic of China—recapitulating the trajectory of Eugene Lyons and William Henry Chamberlin vis-à-vis the USSR—renounced their earlier views and criticized those with whom they once agreed. Ross Terrill, a onetime colleague of Fairbank's at Harvard, recently accused American Sinology of soft-peddling Maoist "social engineering" as well as Mao himself—whom Terrill calls "pathological" and a "borderline personality."⁸²

⁷⁷ Edgar Snow, *The Other Side of the River: Red China Today* (New York, 1962), 619–20; also chap. 81, "Facts about Food"; S. Bernard Thomas, *Season of High Adventure: Edgar Snow in China* (Berkeley, Calif., 1996), 306–08.

⁷⁸ Edgar Snow, *Red Star over China*, rev. and enl. edn. (New York, 1968), 216.

⁷⁹ On the USSR in the 1930s, see, for example, Sir Bernard Pares, "The New Crisis in Russia," *Slavonic and East European Review* 11 (1933): 490; Hans Kohn, "The Europeanization of the Orient," *Political Science Quarterly* 52 (1937): 264. On China in the 1950s, see two retrospectives by American development economists: George Rosen, *Western Economists and Eastern Societies: Agents of Change in South Asia* (Baltimore, 1985); and W. W. Rostow, *Eisenhower, Kennedy, and Foreign Aid* (Austin, Tex., 1985), chaps. 4–6.

⁸⁰ "Wheat and Chaff" [editorial], *Far Eastern Economic Review* 29 (September 29, 1960): 691. While Becker cites this article disapprovingly (*Hungry Ghosts*, 299), he does not put it in the context of that magazine's rather pessimistic view of the Chinese economic plan; the remainder of the editorial, in fact, is a complaint about China's press policies.

⁸¹ On Western responses to the famine of 1958–1960, see Becker, *Hungry Ghosts*, chap. 20; Steven W. Mosher, *China Misperceived: American Illusions and Chinese Reality* (New York, 1990), 110–18; and Article 19, *Starving in Silence: A Report on Censorship and Famine* (London, 1990).

⁸² Ross Terrill, "Mao in History," *National Interest* 52 (Summer 1998): 54–63. For brief analyses of

Yet broader insights from the Soviet famine of 1932–1933 extend well beyond the parallels in China three decades later. The Soviet famine also revealed assumptions about modernization that came to stand at the center of American intellectual life in the decades after World War II. The concepts shaping Western responses to the Soviet famine—enthusiasm for development, reliance on national-character stereotypes, and ideas about “Asia”—operated in various combinations through 1950s and 1960s scholarship in the United States. They appeared at a moment when many intellectuals resolved to grapple with two central problems in the postwar global order: the Soviet Union on the one hand and economic development in the newly independent states of Asia and Africa on the other. The intellectual traffic between scholars of the Soviet Union and of the Third World was heavy, as scholars applied their understanding of Russian/Soviet history to the “battle for the hearts and minds of the Third World.”⁸³

Many scholars made the trip themselves, working in both development studies and Russian/Soviet history. Important figures here include two of the scholars responsible for promoting modernization theory within and outside the academy, Walt Whitman Rostow and Cyril Black, as well as one of the earliest and most incisive critics of modernization, the economic historian Alexander Gerschenkron. All three writers’ works focused on economic aspects of modernization and revolved, to a great degree, around the question of the costs of development. All three drew lessons about such costs from the Soviet case. And in quite different ways, all three scholars employed parts of the logic used by those journalists covering the Soviet famine of 1932–1933.⁸⁴

this shift, see Mosher, *China Misperceived*, 124–38, 177–86; Andrew J. Nathan, “Setting the Scene: Confessions of a China Specialist,” in Nathan, *China’s Crisis: Dilemmas of Reform and Prospects for Democracy* (New York, 1990); and Harry Harding, “The Evolution of Scholarship on Contemporary China,” in *American Studies of Contemporary China*, David Shambaugh, ed. (Washington, D.C., 1993).

⁸³ Nobel laureate W. Arthur Lewis was among those calling attention to this relationship; see *The Theory of Economic Growth* (London, 1955), 431. For more detailed discussions of the Soviet model in development economics, see Morris Watnick, “The Appeal of Communism to the Peoples of Underdeveloped Areas,” *Economic Development and Cultural Change* 1 (1952): 22–36. Also see Francis Seton, “Planning and Economic Growth: Asia, Africa, and the Soviet Model,” *Soviet Survey* 31 (January–March 1960): 48–54; W. Donald Bowles, “Soviet Russia as a Model for Underdeveloped Countries,” *World Politics* 14 (March 1962): 483–504; and Charles K. Wilber, *The Soviet Model and Underdeveloped Countries* (Chapel Hill, N.C., 1969).

On the origins and implications of the term “Third World,” see Martin W. Lewis and Kären E. Wigen, *The Myth of Continents: A Critique of Metageography* (Berkeley, Calif., 1997), 190–93; and especially Carl E. Pletsch, “The Three Worlds and the Division of Social-Scientific Labor, circa 1950–75,” *Comparative Studies in Society and History* 23 (October 1981): 565–90.

⁸⁴ Rostow and Black represent important, economically oriented, strands of modernization theory in the late 1950s and early 1960s, though far from the only ones. Other scholars associated with “modernization” concepts, such as Alex Inkeles, also undertook studies of the Soviet Union. Though Rostow’s original training was in economic history—his PhD dissertation analyzed economic growth in nineteenth-century England—he also wrote a widely circulated book on the Soviet Union (*The Dynamics of Soviet Society* [New York, 1953]); see also W. W. Rostow, “Marx Was a City Boy, or Why Communism May Fail,” *Harper’s Magazine* 210 (February 1955): 25–30. For biographical details on Rostow, see his reminiscences, “Development: The Political Economy of the Marshallian Long Period,” in *Pioneers in Development*, Gerald M. Meiers and Dudley Sears, eds. (Oxford, 1984).

Critiques of modernization theory have been a growth industry in recent years; see Adas, *Machines as the Measure of Man*, 402–18; Ian Roxborough, “Modernization Theory Revisited: A Review Article,” *Comparative Studies in Society and History* 30 (1988): 753–61; and especially Michael Edward Latham, “Modernization as Ideology: Social Science Theory, National Identity, and American Foreign Policy,

The three scholars derived from the Russian experience lessons for Third World development. Gerschenkron first discussed his theory of “economic backwardness” at a 1951 conference called “The Progress of Underdeveloped Areas.” He originally submitted his contribution as “Historical Bases of Appraising Economic Development in a Bipolar World,” a title that emphasized the connection between Third World development and Soviet-American antagonisms. The conference organizer, wary of promoting a paper with such a wordy title, proposed an alternative, which was later to earn Gerschenkron his reputation: “Economic Backwardness in Historical Perspective.” That article drew lessons from Russia’s “extreme relative backwardness” to show how such economies required significant levels of state intervention in order to industrialize.⁸⁵

Rostow, like Gerschenkron, drew lessons from the Russian experience for former colonies just beginning the process of development. Rostow’s *Stages of Economic Growth* (1960)—subtitled “A Non-Communist Manifesto”—aimed to discredit the Soviet approach to industrialization. Yet the Russian experience was a touchstone throughout the book. His description of the role of the agricultural sector in industrialization fit the Soviet experience especially well: the rural sector provided foreign exchange (via the export of primary commodities) and government funds to promote industry (via taxes).⁸⁶

Black’s transit between the Russian past and the Third World present was even more frequent. Starting in 1960, he promoted the application of modernization theory to the study of Russian history by editing a conference volume, *The Transformation of Russian Society*. It included contributions by Gerschenkron, Talcott Parsons, and an impressive array of present and future leaders in Russian/Soviet studies. By the mid-1960s, he had expanded his field of inquiry dramatically, publishing a world-history primer called *The Dynamics of Modernization* (1966); its opening pages asserted that modernization was the third fundamental transformation of life on earth, on par with the rise of human life and the shift to settled agricultural societies.⁸⁷

1961–1963” (PhD dissertation, University of California, Los Angeles, 1996, forthcoming, Chapel Hill, N.C., 2000). I have also learned much about the meanings of modernization theory from ongoing discussions and disagreements with Nils Gilman, who is currently completing his dissertation on the topic.

⁸⁵ On the paper’s title, see the exchanges between Gerschenkron and conference organizer Bert Hoselitz in Box 8, series HUG 45.10, Alexander Gerschenkron Papers, Pusey Library, Harvard University. The article appeared originally in *The Progress of Underdeveloped Areas*, Bert F. Hoselitz, ed. (Chicago, 1952), and was later rpt. in Gerschenkron, *Economic Backwardness in Historical Perspective: A Book of Essays* (Cambridge, Mass., 1962), and in many other collections. An excellent summary and critique of Gerschenkron’s scholarship is offered by a former student: D. N. McCloskey, “Kinks, Tools, Spurts, and Substitutes: Gerschenkron’s Rhetoric of Relative Backwardness,” in *Patterns of Industrialization: The Nineteenth Century*, Richard Sylla and Gianni Toniolo, eds. (London, 1991), 92–107.

⁸⁶ W. W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge, 1960), esp. 22–24; this relationship between agriculture and industry, of course, did not fit only the USSR. Rostow began working out these stages in a more abstract work, *The Process of Economic Growth* (New York, 1952).

⁸⁷ Cyril E. Black, “The Modernization of Russia,” in Black, ed., *The Transformation of Russian Society: Aspects of Social Change since 1861* (Cambridge, Mass., 1960); see also Black’s essay, “The Nature of Imperial Russian Society,” along with commentary by Hugh Seton-Watson and Nicholas V. Riasanovsky in *Slavic Review* 20 (December 1961): 565–600. Black’s key work on modernization theory

Of these three postwar scholars, Black had by far the most in common with the journalists of the 1930s. First, he employed national-character traits with a frequency similar to Duranty, Chamberlin, Fischer, and Lyons. His introductory essay to the Russian-history volume stressed Russians' passivity, patience, and submissiveness. Like the earlier journalists, Black deployed these traits in the interests of economic development. The traits explained the combination of economic hardship and political coercion that defined the Soviet era. Russians' traditional fatalism—which, Black claimed, began to wane only after World War II—explained the ease with which Soviet authorities could maintain their rule.⁸⁸ Finally, Black shared the journalists' argument that progress would be costly, even fatal, but was ultimately necessary. Considering modernization "simultaneously creative and destructive," Black hardly hid the costs, arguing that violence in modernization was primarily the result of the "radical character of the changes inherent in modernization." Twenty years later, only months after Gorbachev took power in the Soviet Union, Black described economic change as a worthy justification for despotic rule: "We think autocracy is a bad thing. It crushes individuals. Yet in real life you have to get things done. You have to get organized. And the Russians from that point of view had a good government, an effective government, with all its shortcomings—and still [do]."⁸⁹ Black envisioned modernization as a process through which all societies must pass; the degree of violence varied by political circumstances and especially "national character." The universalism of this vision is perhaps best exemplified by his conjecture that the process of modernization might eventually lead to a world so homogeneous that a "single world state" would emerge.⁹⁰

Rostow shared Black's universalism; his *Stages* insisted that all nations went through a similar process of modernization, differing primarily in timing. He had even less room for variation than did Black. Rostow agreed with Black about the sacrifices and even violence that could accompany the social transformations they both described. By the late 1950s, Rostow had worked out foreign-policy applications of his theory, centered on the costs of modernization. Economic change, he argued, "create[d] potential unrest by dislodging convictions and habit patterns which have in the past insured stability." Such instability would make developing nations vulnerable to propagandizing by communists, whom Rostow called "scavengers of the modernization process." He devoted a significant portion of his political career, as a foreign-affairs adviser to presidents John F. Kennedy and Lyndon Baines Johnson, to establishing and implementing aid programs that would

is *The Dynamics of Modernization: A Study in Comparative History* (New York, 1966). A similarly ambitious claim for modernization theory, that every problem in the world is related to modernization, is made in Marion J. Levy, Jr., *Modernization: Latecomers and Survivors* (New York, 1972), 1.

⁸⁸ Cyril E. Black, "The Modernization of Russia," in Black, *Transformation of Russian Society*, 667, 672, 679. See also Black, "Russian History and Soviet Politics," testimony to the Subcommittee on Strategic Arms Limitation Talks of the Committee on Armed Services, March 18, 1970, rpt. in Black, *Understanding Soviet Politics: The Perspective of Russian History* (Boulder, Colo., 1986), 14–15.

⁸⁹ Black, *Dynamics*, 27, 33, 159. "Tradition and Modernity," lecture to the Summer Institute on "Global Interdependence and New Jersey Education," July 15, 1985, Box 4, Cyril E. Black Papers, Firestone Library, Princeton University.

⁹⁰ Black, *Dynamics*, 164. In that work, Black outlined a taxonomy of seven types of modernization based primarily on timing and political systems; see 106–28.

help prevent communist “scavengers” from taking advantage of the turmoil. This was a significant and explicit goal of the international “Economic Development Decade” he spearheaded in the 1960s.⁹¹

Gerschenkron’s dissent from Rostow’s and Black’s universalism would eventually reveal an important application of cultural particularism. Gerschenkron’s theory of economic backwardness combined a heuristic for understanding industrialization with a method for explaining differences based on the degree of “relative backwardness.” The state role necessary for the most backward nations would eventually wither away, Gerschenkron believed, as once-backward nations began to lessen the gap between “what is” and “what could be.” Not prone to the national-character stereotyping so common among the 1930s journalists, the economic historian nevertheless incorporated one of their most important themes: the notion that Russian development was “Asian.” While Rostow and Black saw modernization as a universal process oftentimes indistinguishable from westernization (in Rostow’s case, Americanization), Gerschenkron instead insisted on the separability of economic from political progress. The emergence of an industrial economy—the process he called westernization—could happen through the most “unwestern” means. Russia, to Gerschenkron, provided an excellent case: the Soviets’ “westernization of the economy,” he wrote, was accompanied by its political “orientalization.” “Asia” thus took on different meanings: in the political sphere, it implied repression and despotism; in the economic sphere, it suggested stagnation and resistance to development. The result, what he saw as Russia’s combination of economic advancement and political retrogression, nevertheless met its desired aims. While disparaging the Soviet “dictatorship,” Gerschenkron conceded that it “no doubt has contributed to the course of industrialization more than any other single factor.”⁹²

Comparing Gerschenkron to Rostow and Black thus reveals interesting points of intersection and difference. Those endorsing the universalism of modernization theory argued that all nations undergo a similar process of social transformation and end up in a similar state: as an “integrated society” (Black) or in the “age of high-mass consumption” (Rostow). Like the journalists covering the Soviet famine of 1932–1933, Black and Rostow recognized the costs entailed but called for further modernization. Black carried the parallel with the journalists of the 1930s further, stressing that such costs relate to national-character traits. Rostow was swayed more by national interest than national character; he called for American aid to prevent the virus of communism from affecting those paying the costs. Gerschen-

⁹¹ W. W. Rostow and Max F. Millikan, *A Proposal: A Key to an Effective Foreign Policy* (New York, 1957), 22. This book represented Rostow’s first major effort to apply his development theories to foreign policy. For the connections between Rostow’s economic scholarship and foreign policies (especially in the Kennedy White House), see John Lodewijks, “Rostow, Developing Economies, and National Security Policy,” in *History and Political Economy*, Annual Supplement, 23 (1991): 285–310; and Latham, “Modernization as Ideology.”

⁹² Alexander Gerschenkron, “The Early Phases of Industrialization in Russia: Afterthoughts and Counterthoughts,” in *The Economics of Take-Off into Sustained Growth: Proceedings of a Conference Held by the International Economic Association*, W. W. Rostow, ed. (London, 1963), 155–56; Gerschenkron, “Problems and Patterns of Russian Economic Development,” in Black, *Transformation of Russian Society*, 71; Gerschenkron, *Economic Backwardness*, 186; Gerschenkron, review of *The Socialized Agriculture of the USSR*, by Naum Jasny (1950), in Gerschenkron, *Continuity in History and Other Essays* (Cambridge, Mass., 1968), 481.

kron, on the other hand, did not universalize the process of modernization, insisting instead on significant variations in national experiences. He also defined progress as divisible—unlike Rostow and Black—and proposed that the Soviet Union represented economic progress enacted through political retrogression. Gerschenkron characterized such cases by employing the language of “Asia” so central to both Chamberlin and Duranty. To Gerschenkron, “Asia” defined a particular form of social change, a form found in early Soviet Russia.⁹³ None of these three postwar scholars went as far as the 1930s journalists in offering justifications for Soviet economic policy in the era of the first Five-Year Plans. Yet all of them incorporated one or another aspect of American journalists’ explanation of causes and consequences of the famine. That such usage entered mainstream intellectual life, as a central project of Western social science in the 1960s, suggests the durability of combining “national character” and economic development. Even though modernization theory fell out of academic favor in the late 1960s and beyond, the issues have hardly faded away.

THE IDEA OF A UNIQUELY ASIAN VARIANT of economic development was reappropriated by political leaders behind Asia’s “economic miracle” of the early 1990s, including Singapore’s Lee Kuan Yew and Indonesia’s Raden Suharto. These leaders saw rapid industrialization under political authoritarianism as the expression of “Asian values,” thus turning “Oriental despotism” from a criticism to a compliment. Critics of Singapore and Indonesia, however, had a different name: “development dictatorships.” Differences in terminology expressed radically different resolutions of the tension between the goal of economic development and the methods used to attain that goal—a central tension for the field of development economics.⁹⁴

Notions of the “Asiatic” eased this tension for many Western experts on

⁹³ The frequent application of “oriental despotism” to Russia and the Soviet Union deserves a fuller discussion than can be provided here. See, of course, Karl A. Wittfogel, *Oriental Despotism: A Comparative Study of Total Power* (New Haven, Conn., 1957), and the rebuttal of sorts in Lewis and Wigen, *Myth of Continents*, 93–97. The connections between Wittfogel’s writings and Russian discourses of “Asia” are explored in detail in G. L. Ulmen, *The Science of Society: Toward an Understanding of the Life and Work of Karl August Wittfogel* (The Hague, 1978), 245–61, 352–54. For longer-term historical roots, see Wolff, *Inventing Eastern Europe*; and Donald M. Lowe, *The Function of “China” in Marx, Lenin, and Mao* (Berkeley, Calif., 1966).

⁹⁴ Amartya Sen, “Human Rights and Asian Values: What Lee Kuan Yew and Le Peng Don’t Understand about Asia,” *New Republic* 217 (July 14, 1997): 33–40; Sen, “Freedom Favors Development (Elections after the End of History),” *New Perspectives Quarterly* 13 (Fall 1996): 23–27; Sen, “Liberty and Poverty: Political Rights and Economics,” *Current* (May 1994): 22–28. See also a special issue of *Journal of Democracy* (8 [April 1997]) devoted to the topic “Hong Kong, Singapore, and ‘Asian Values.’” The Asian financial crisis of the winter of 1997–1998 prompted other criticisms of “Asian values”: Francis Fukuyama, “Asian Values and the Asian Crisis,” *Commentary* 105 (February 1998): 23–27; Milton Friedman, “Asian Values: Real Lesson of Hong Kong,” *National Review* 49 (December 31, 1997): 36–37.

Among the best summaries of development economics are those by participants; see especially Albert O. Hirschman, “The Rise and Decline of Development Economics,” in his *Essays in Trespassing: Economics to Politics and Beyond* (Cambridge, 1981); and H. W. Arndt, *Economic Development: The History of an Idea* (Chicago, 1987). For recent works, see, for instance, Colin Leys, *The Rise and Fall of Development Theory* (Bloomington, Ind., 1996); and especially the contributions in *International Development and the Social Sciences: Essays on the History and Politics of Knowledge*, Frederick Cooper and Randall Packer, eds. (Berkeley, 1997).

economic change. Observers used the appellation to imply individuals' passivity, fatalism, and general unsuitability for modern economic life. Centuries-old conceptions of "Oriental despotism"—like more recent claims about "Asian values"—offered a ready explanation for economic development under strict political regimes. Interwar Russia experts, many of whom saw Russia as an "Asiatic" society, applied a similar logic. Their responses to the Soviet famine of 1932–1933 reveal the potency of combining such national-character stereotypes with a belief in economic development at all costs. Walter Duranty and Louis Fischer, so often blamed for the lack of major coverage of the famine, shared most of these ideas with their chief critics, Eugene Lyons and William Henry Chamberlin. These four reporters invoked peasant "passivity" and "apathy" as innate personal characteristics, not just responses to circumstances of collectivization, to explain and perhaps even justify the devastation of the Soviet countryside.

Thirty years later, Western academics turned their attention to economic modernization of the former colonies and once again considered the relationship between "national character" and economic development. Cyril Black used individual Russian traits such as passivity to explain the nation's economic and political trajectories. Alexander Gerschenkron applied notions of "Asiatic" Russia to suggest the separability of economic and political development. While recognizing the human costs of economic change, Walt Rostow insisted on its ultimate benefits, revealing the continuing power of what George F. Kennan called "the romance of economic development." Kennan coined that phrase in 1932 to explain why Soviet youths were willing to tolerate great sacrifices during the first Five-Year Plan. Yet the romance held people of all nations under its sway—observers trying to explain industrialization's high costs as well as activists willing to endure those costs (and inflict them on others). Echoing Kennan's words a quarter-century later, future U.S. national security adviser Zbigniew Brzezinski wrote that university youths and other intellectuals "appear to be hypnotized by the image of large-scale industry."⁹⁵ Although both Kennan and Brzezinski focused on young people (their seduction by economic development or their hypnosis by large factories), such affairs of the heart were not mere teen infatuations. Economic development in the twentieth century was both made and understood by political leaders and intellectuals under a similar spell.

⁹⁵ Zbigniew Brzezinski, "The Politics of Underdevelopment," *World Politics* 9 (October 1956): 60.

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A Time of Reconquest: History, the Maya Revival, and the Zapatista Rebellion in Chiapas

THOMAS BENJAMIN

We are in the new Time of the Mayas.¹

ON THE AFTERNOON OF October 12, 1992, a large protest march of indigenous peoples in the colonial city of San Cristóbal de las Casas, in the southeastern Mexican state of Chiapas, reached its objective. In the courtyard of the beautiful baroque temple of Santo Domingo was the monument to the conquistador Diego de Mazariegos, founder of the city. One marcher knocked the statue off its base with a sledge hammer, and the crowd then beat it into fragments. Hundreds of marchers returned to their mountain homes with souvenirs of an unforgettable historical event. After surviving five centuries of systemic violence and exploitation, the natives of the highlands of Chiapas destroyed the premier symbol of their oppression. This event, along with other recent dramatic actions, has led one observer to remark that "they are living in a time of reconquest in Chiapas."²

This small episode by people long scorned and exploited in a remote corner of the world provides an interesting perspective on the blurred boundary between thinking about history and making history, between history as knowledge and history as event.³ Those who erected the monument to Mazariegos inherited and cultivated a particular interpretation of the past that was represented in that

I would like to thank Jan and Diane Rus, who made this essay possible by providing me with many of the publications of the Tzotzil Workshop, facilitating interviews in San Cristóbal, and trying to educate me about native Chiapas over the years. I am grateful to those Chiapanecos who welcomed my visits and agreed to talk with me about this topic and this beautiful but difficult land that Bishop Samuel Ruiz once called the Mexican frontier with the past. I would also like to acknowledge the valuable conversations I have had with Andrés Aubry, José Jimenez Paniagua, Neil Harvey, Christine Eber, and Monique Nuijten. I am grateful to Jan Rus, Paul Vanderwood, Carol Green, Michael Grossberg, and the five anonymous reviewers of this journal for their insightful comments on earlier versions of this article. Central Michigan University supported my research and writing with a research professorship.

¹ From a one-act farce adapted by Francisco Alvarez Quiñones of the native Writers' Cooperative (Sna Itz'ibajom) of San Cristóbal de las Casas, "Tiempo de los Mayas," *Centro de Investigaciones Humanísticas de Mesoamérica del Estado de Chiapas* (hereafter, *CHIMECH*) 4 (January–December 1994): 197.

² Adriana López Monjardín, "Los guiones ocultos de Chiapas: La resistencia cívica entre los indígenas," *Viento del sur* 7 (Summer 1996): 23.

³ "Our argument about past and present points to the unity of history and politics, to historical work as an aspect of politics." Popular Memory Group, "Popular Memory: Theory, Politics, Method," in Richard Johnson, et al., eds., *Making Histories: Studies in History Writing and Politics* (Minneapolis, 1982), 244. The word "history" has more than one meaning in common usage. In this essay in most instances, the word refers to its conventional meaning, the recording, analyzing, narrating, and

monument. Theirs is a historical society, interested in their past, sanctioned by the past, and some might say, obsessed with the past. Those who destroyed the monument, in contrast, have long been considered a "people without history." Their action on October 12, however, suggests otherwise.⁴

The concept of a "people without history" has two meanings in this setting: one is historiographical, the other is philosophical. Indians of this region since the Conquest have not produced written chronicles and histories of their own. The history of Chiapas has been written by Mexican, European, and U.S. historians. Indians did not disappear from the pages of history; rather, they were simply not perceived, as Eric Wolf put it, "as participants in the same historical trajectory."⁵ In written history—colonial and later national history—Indians after the Conquest ceased to be the protagonists in their own story. Spaniards and later Mexicans portrayed Indians, at best, as passive victims and inert obstacles to progress (that is, "to the course of history") and, more generally, as irrational and uncivilized. Modern Mexican nationalism today proudly lays claim to the nation's ancient Indian heritage and proclaims its Indian peoples to be the soul of the nation.⁶ Yet, until quite recently, Indians have been installed below the surface of Mexican

explaining of past events and processes or simply knowledge of the past. Another meaning of the word is that of the flow of events in the past, present, and future, as in Oscar Wilde's saying that any fool can make history but it takes a genius to write it. Richard J. Evans, *In Defense of History* (1997; New York, 1999), 173.

⁴ There are heated debates about the division of the world into peoples with and without history. One concerns the validity of the distinction itself and has become a fierce ideological issue. Revisionists question the "stereotype" of ahistorical myth and legend and argue that every dominant ideology declares "the other" to have neither history nor historical understanding. Much good work has discredited the older, condescending treatment of non-elite traditions and sensitized historians to the different ways people have looked at and understood their past. See Romila Thapar, "Society and Historical Consciousness: The Itihasa-Purana Tradition," in Subysachi Bhattacharya and Thapar, eds., *Situating Indian History* (New Delhi, 1986), 353–84. The other debate in simplified form concerns the value of Western historicity. Claude Lévi-Strauss viewed mythic thought as valuable and authentic and the European imposition of history as the obliteration of cultural difference and one more tool of human enslavement. Traditional Eurocentric analysis considered the rise of historical consciousness as part of the march of progress, while more recently Jacques Derrida suggested the possibility of historical consciousness as necessary for liberation. It is not my intention to enter this discussion but to consider the more limited question of how a specific people began to put into writing their own historical narratives. See Claude Lévi-Strauss, *Tristes Tropiques: An Anthropological Study of Primitive Societies in Brazil*, John Russell, trans. (New York, 1969); Jacques Derrida, *Of Grammatology*, Gayatri Spivak, trans. (Baltimore, 1976); and Kerwin Lee Klein, "In Search of Narrative Mastery: Postmodernism and the People without History," *History and Theory* 34 (1995): 275–98.

⁵ Eric R. Wolf, *Europe and the People without History* (Berkeley, Calif., 1982), 23. Native peoples, like E. P. Thompson's working class, experienced "the enormous condescension of posterity." Thompson, *The Making of the English Working Class* (New York, 1963), 12. Also see Ralph Buultjens, "Global History and the Third World," in Bruce Mazlish and Buultjens, eds., *Conceptualizing Global History* (Boulder, Colo., 1993), 71–91. Richard White notes that "the oldest and most lasting tradition in the representation of Indians" is that they are "a people without history." See "Representing Indians," *New Republic* (April 21, 1997): 32.

⁶ The best review and analysis of this historiographical domination is Guillermo Bonfil Batalla, *México Profundo: Reclaiming a Civilization*, Philip A. Dennis, trans. (Austin, Tex., 1996). Also see Bonfil Batalla, "Historias que no son todavía historia," in Carlos Pereyra, et al., *Historia ¿Para Que?* (Mexico City, 1980), 229–45; Bonfil Batalla, "Nuestro patrimonio cultural: Un laberinto de significados," in *El patrimonio cultural de México*, Enrique Florescano, ed. (Mexico City, 1993), 19–39; Francisco de la Peña Martínez, "La construcción imaginaria de la mexicanidad," *La jornada semanal* 212 (July 4, 1993): 31–34; and Luis Reyes García, "Comentarios sobre historia india," in *Movimientos indígenas contemporáneos en México*, Arturo Warman and Arturo Argueta, eds. (Mexico City, 1993), 187–98.



"12 de Octubre de 1992 día de la raza [Day of the Race]." Marchers reach the monument to conquistador Diego de Mazariegos on October 12, 1992. The photographer wishes to remain anonymous.

historiography by its makers; like the half-buried Mesoamerican pyramid, they appear to be there, ancient, enduring, but frozen in time.⁷

The modern Maya of Chiapas have been considered a "people without history"

⁷ Thomas Benjamin, "Una larga historia de resistencia indígena campesina: Un ensayo sobre la etnohistoriografía de Chiapas," in Jane-Dale Lloyd and Laura Pérez Rosales, eds., *Paisajes rebeldes: Una larga noche de rebelión indígena* (Mexico City, 1995), 183–85.

in the other meaning of the phrase: they are supposedly a people of myth who lack a historical consciousness.⁸ Ancient Maya hieroglyphic texts, recently deciphered, reveal the dynastic histories of rulers and thus demonstrate the existence of a written Maya historical tradition.⁹ But most scholars have reported a cyclical rather than a linear temporal order in the abundant sacred narratives and secular stories that make up the oral tradition of the native peoples of the region. The perspectives reported by anthropologists were not, of course, the timeless primordial cosmologies of the ancient Maya preserved in traditional culture. Narratives were of mythic origins, epic events, and futures foretold. The remote past was telescoped into the present, while events were related in a distorted temporal fashion or presented outside of time altogether. Stories were populated by supernatural beings, anthropomorphic animals, and human actors. Such tales of the past were believed to be true but were not concerned with realistic representation of the past. They belonged within “the timeless paradigm of myth and ritual.”¹⁰

In 1966, Benjamin N. Colby reported, “we have found little in the way of historical knowledge in Zinacantan stories of myths, least of all any explicit references to the conquest.” Regarding the dominant culture, specifically San Cristóbal, he observed, “in Ladino [non-Spanish] society there is, on the contrary, a very strong historical awareness extending all the way to the time of the conquest.”¹¹ The Indians of Chiapas in the 1960s were undoubtedly a people without history (in both meanings) in the view of the new bishop of San Cristóbal de las Casas. “They didn’t have a history,” he stated in a 1994 interview, “because

⁸ What John Lukacs has called “unhistorical habits of thought”—and others call non-Western historical consciousness—is, in his formulation, a different form of consciousness in contrast to the Greek and European tradition of realistic representation of the past and the belief that anything—a person, a nation, even an idea—can be known through its history. See Lukacs, *Historical Consciousness: The Remembered Past* (1968; New Brunswick, N.J., 1994), chap. 1, sect. 5; Ernst Breisach, *Historiography: Ancient, Medieval, and Modern* (Chicago, 1983); Herbert Butterfield, *The Origins of History* (London, 1981); Robert Eric Frykenberg, *History and Belief: The Foundations of Historical Understanding* (Grand Rapids, Mich., 1996). Some recent scholars of Native American history agree that the Indian understanding of time is fundamentally different from, and possibly superior to, the Western idea of history. Calvin Martin makes a clear distinction between ‘people of myth’ and ‘people of history’ and advises historians against seeing or portraying Native Americans as a people of history. See Martin, ed., *The American Indian and the Problem of History* (New York, 1987). Kerwin Lee Klein discusses the new criticism in “In Search of Narrative Mastery,” 275–98; and *Frontiers of Historical Imagination: Narrating the European Conquest of Native America, 1890–1990* (Berkeley, Calif., 1997), 287–96.

⁹ Robert M. Carmack, Janine Gasco, and Gary H. Gossen, *The Legacy of Mesoamerica: History and Culture of a Native American Civilization* (Upper Saddle River, N.J., 1996), 455.

¹⁰ Victoria Reifler Bricker, *The Indian Christ, the Indian King: The Historical Substrate of Maya Myth and Ritual* (Austin, Tex., 1981), 180. Miguel León-Portilla considered recent ethnologies and historiography in *Tiempo y realidad en el pensamiento Maya: Ensayo de acercamiento*, 3d edn. (Mexico City, 1994), 174–92.

¹¹ Benjamin N. Colby, *Ethnic Relations in the Chiapas Highlands of Mexico* (Albuquerque, N.Mex., 1966), 20. Similar findings are given by Fernando Cámara Barbachano, *Persistencia y cambio cultural entre los tzeltales de los altos de Chiapas* (Mexico City, 1966); Henri Favre, *Cambio y continuidad entre los Mayas de México: Contribución al estudio de la situación colonialista en América Latina* (Mexico City, 1973), 130–31, 88; Calixta Guiteras-Holmes, *Perils of the Soul: The World View of a Tzotzil Indian* (Chicago, 1961); William R. Holland, *Contemporary Tzotzil Cosmological Concepts as a Basis for Interpreting Prehistoric Maya Civilization*, 35th International Congress of Americanists, 3 vols. (Mexico City, 1962); Ricardo Pozas, *Juan Pérez Jolote: Biografía de un Tzotzil* (Mexico City, 1952); June Nash, *In the Eyes of the Ancestors: Belief and Behavior in a Maya Community* (New Haven, Conn., 1970); Carlos Navarrete, *Chiapanec History and Culture*, José Gabriel Camacho, trans. (Provo, Utah, 1966); and Evon Z. Vogt, *Tortillas for the Gods: A Symbolic Analysis of Zinacantan Rituals* (Cambridge, Mass., 1976).

those who are dominated do not have a history, it's the history of the dominator. How could we lay a foundation for a church where there wasn't even a historical consciousness."¹²

The modern Maya sense of the past, according to the recent work of scholars of Maya oral tradition and history, is more complicated than many had thought. Gary H. Gossen found in his 1974 study of Chamula, a highland Indian municipality in Chiapas and neighbor of Zinacantán, a "true ancient narrative" that includes "much of what is frequently glossed as myth, legend, and folktale" and a "true recent narrative" that encompasses generational and historical memory extending 120 to 150 years into the past. Narratives of the latter category referred to historical events but were never precisely dated or necessarily placed in chronological order. The past was very much like the present, not "another country," and focused on and confined to the traditionally closed highland community.¹³

The ongoing work of Jan Rus regarding the highland Tzotzil is among the first historical research based largely on "true recent narratives." His interviews with native informants and careful analysis of published folk tales (and correlation with documentary evidence) demonstrate the considerable historical knowledge in native communities available to someone who has the trust of his informants and, more important, knows how to ask the right questions.¹⁴ The rebel leader known only as Subcomandante Marcos, who has lived among Indians in Chiapas since the early 1980s, discovered that native villages each designated a historian, who inherited the memory of the community. These historians, even if young men, he reported, could tell of specific events long past in considerable detail as if they had been there.¹⁵ The work of Gossen and Rus, and the observations of Marcos, demonstrate that the dichotomy of peoples of history and peoples of myth is facile and false. There is no sharp line separating mythical and historical understanding

¹² Paulina Hermosillo, "Interview: Bishop Samuel Ruiz García," in Elaine Katzenberger, ed., *First World, Ha Ha Ha! The Zapatista Challenge* (San Francisco, 1995), 72. The bishop recognizes a significant transformation in this respect. In an interview in 1997, he noted, "the Indians are no longer objects. They have become the subjects of their lives. They no longer see things mundane or divine as they did before . . . They are making new interpretations of their old culture." From John Womack, Jr., "A Bishop's Conversation," *DoubleTake* 4 (Winter 1998): 27.

¹³ Gary H. Gossen, *Chamulas in the World of the Sun: Time and Space in a Maya Oral Tradition* (Cambridge, Mass., 1974), 25, 140–41, 253–54. Gossen has a large and distinguished body of published work. Most relevant to this discussion is "El tiempo cíclico en San Juan Chamula: Mistificación o mitología viva?" *Mesoamerica* 18 (December 1989); and "Translating Cuscat's War: Understanding Maya Oral History," *Journal of Latin American Lore* 3 (1977): 249–78.

¹⁴ Jan Rus, "Contained Revolutions: Indians and the Struggle for Control of Highland Chiapas, 1910–1925," unpublished paper; Rus, "Whose Caste War? Indians, Ladinos and the Chiapas 'Caste War' of 1869," in Murdo J. MacLeod and Robert Wasserstrom, eds., *Spaniards and Indians in Southeastern Mesoamerica: Essays on the History of Ethnic Relations* (Lincoln, Neb., 1983), 127–68; Rus, "The 'Caste War' of 1869 from the Indian's Perspective: A Challenge for Ethnohistory," *Memorias del Segundo Coloquio Internacional de Mayistas* (August 17–21, 1987), vol. 2 (Mexico City, 1989), 1033–47; Rus, "The 'Comunidad Revolucionaria Institucional': The Subversion of Native Government in Highland Chiapas, 1936–1968," in Gilbert M. Joseph and Daniel Nugent, eds., *Everyday Forms of State Formation: Revolution and the Negotiation of Rule in Modern Mexico* (Durham, N.C., 1994), 265–300; and Rus, "Local Adaptation to Global Change: The Reordering of Native Society in Highland Chiapas, Mexico, 1974–1994," *European Review of Latin American and Caribbean Studies* 58 (1995): 71–89.

¹⁵ Interview with Subcomandante Marcos conducted by Carmen Castillo and Tessa Brisac, Aguascalientes, Chiapas, October 24, 1994, in Adolfo Gilly, Subcomandante Marcos, and Carlo Ginzburg, *Discusión sobre la historia* (Mexico City, 1995), see "Apéndice: Historia de Marcos y de los Hombres de la Noche," 131–42.

in Maya culture or in any culture. They coexist and, at times, overlap.¹⁶ There is no essentialist or traditional worldview, furthermore, that makes narration of past events impossible.¹⁷

The modern Maya of Chiapas, until recently, have been a people without written history and thus without a useful past that would encourage pan-Mayan organization and action, make them an integral part of the Mexican nation, and empower them in their own eyes and in the eyes of society. The dominant culture made it so and found it to be agreeable for a very long time. "For so many years," Eugenio Maurer writes, "the idea was hammered in that Tseltal culture was worthless, idolatrous and superstitious."¹⁸ The so-called bilingual and bicultural government-supported indigenous schools from the 1950s to the present "treated indigenous laws, methods of social organization, lifestyles, and art forms as if they did not exist, or were not worthy of study or emulation."¹⁹ Indigenous school teachers in Oxchuc taught history "but not their own local history as Maya people."²⁰ In time, this absence of teaching and writing indigenous history was understood by natives themselves as problematic. A 1974 indigenous congress in Chiapas recognized the necessity of studying and teaching the history and customs of Indians in order to create a new indigenous identity for themselves and within Mexican society. "The Indian peoples *need* to know their own history," Guillermo Bonfil Batalla wrote in 1980. "This is imperative given their present struggles because their demands are based precisely on the affirmation of their historical legitimacy as peoples."²¹

During the 1970s and 1980s, an Indian revitalization movement emerged not only in Chiapas but throughout Mexico and the Americas.²² In Chiapas as elsewhere, this movement has been characterized by efforts to encourage cultural vitality as well as promote political, agrarian, and labor activism and organization-building

¹⁶ Bricker, *Indian Christ, the Indian King*, 180; Carol Karasik, ed., *The People of the Bat: Mayan Tales and Dreams from Zinacantan*, collected and translated by Robert M. Laughlin (Washington, D.C., 1988), 1–21; and J. M. Levi, "Myth and History Reconsidered: Archeological Implications of Tzotzil-Maya Mythology," *American Antiquity* 53 (July 1988): 605–10. Oral tradition, we should also recognize, is a verbal art, not a precise referencing system. See Jan Vansina, *Oral Tradition: A Study in Historical Methodology*, H. M. Wright, trans. (Chicago, 1965), 183–85, 102.

¹⁷ Joanne Rappaport has made the most in-depth study of a native historical tradition, that of the Nasa of Colombia, and found a distinct vision that merged myth and history. See Rappaport, *The Politics of Memory: Native Historical Interpretation in the Colombian Andes* (Durham, N.C., 1998).

¹⁸ Eugenio Maurer, "Tseltal Christianity," in Manuel M. Marzal, Maurer, Xavier Albó, and Bartomeu Meliá, *The Indian Face of God in Latin America*, Penelope R. Hall, trans. (New York, 1996), 62.

¹⁹ Stephen E. Lewis, "Revolution and the Rural Schoolhouse: Forging State and Nation in Chiapas, Mexico, 1913–1948" (PhD dissertation, University of California, San Diego, 1997), 446.

²⁰ Carlos R. Vargas Morales, "La lingüística antropológica aplicada, Oxchuc, altos de Chiapas," *Memorias del Primer Congreso Internacional de Mayistas* (Mexico City, 1992), 125. Nancy Modiano in 1973 found that the only history in Indian schools was "polemics about national heroes . . . which were all but meaningless to the students." Modiano, *Indian Education in the Chiapas Highlands* (New York, 1973), 104.

²¹ Bonfil Batalla, "Historias que no son todavía historia," 244. "History is a question of power in the present, and not of detached reflection upon the past. It can serve to maintain power, or can become a vehicle for empowerment." Rappaport, *Politics of Memory*, 16.

²² Marie-Chantal Barre, *Ideologías indigenistas y movimientos indios* (Mexico City, 1983); Héctor Díaz Polanco, *Indigenous Peoples in Latin America: The Quest for Self-Determination*, Lucia Rayas, trans. (Boulder, Colo., 1997), see chap. 5, 83–93. Anthony Wallace defines revitalization movements as "deliberate, organized, conscious effort(s) by members of a society to construct a more satisfying culture." Wallace, "Revitalization Movements," *American Anthropologist* 58 (1956): 265.

encompassing different ethnic communities.²³ The writing of history is part of the Maya revival, and this essay examines the origins of this cultural innovation. An indigenous historiography in native languages by native historians has appeared in print for the first time. This new historiography rejects the long dominant historical perspective that denied indigenous resistance to domination and exploitation as well as Mexico's multiethnic and multicultural nature. This new historiography presents the Maya as protagonists, not passive victims in the past, promotes a pan-Maya identity in the present, and places the Maya in the national story that is Mexican history. By becoming their own historians, the Maya demonstrate that they are a people, and that they are Mexicans, with the right and ability of self-representation and self-determination. History is one of the new initiatives taken, Bonfil Batalla recognized, "to recover and modernize Indian cultures."²⁴

A year and a few months after the fall of the conquistador statue in San Cristóbal, an indigenous uprising in Chiapas startled Mexico and the world. The Zapatista Army of National Liberation, whose members numbered perhaps 2,000 soldiers with a popular base of many thousands, seized San Cristóbal, Ocosingo, Altamirano, and Las Margaritas on New Year's Day 1994. The guerrilla army quickly retreated into the remote valleys and forests of eastern Chiapas as the Mexican army poured more than 12,000 troops into the state. The political, social, and economic roots of the Zapatista rebellion have begun to be investigated. The influence of radical activists and an activated clergy among poor peasant farmers is being examined.²⁵ This essay considers one aspect of the Maya revival and the Zapatista rebellion that has not received scholarly attention: how the Maya are becoming their own historians.

SAN CRISTÓBAL DE LAS CASAS in colonial times was a small Spanish island within a vast Indian sea. The proportions have not changed.²⁶ The highlands of Chiapas are dotted with thousands of small native hamlets and towns, which are home to several hundreds of thousands of predominantly Mayan peoples. They are poor peasant

²³ June Nash, "The Reassertion of Indigenous Identity: Mayan Responses to State Intervention in Chiapas," *Latin American Research Review* 30 (1995): 7–41.

²⁴ Bonfil Batalla, *México Profundo*, 149. "Throughout the Americas indigenous peoples are working toward these same aims, revalidating their own historical knowledge as an arm against their subordinate position in society. For them, history is a source of knowledge of how they were first subjugated and of information about their legal rights, the beginnings of a new definition of themselves as a people." Rappaport, *Politics of Memory*, 1.

²⁵ For the early historiography, see Barry Carr, "'From the Mountains of the Southeast': A Review of Recent Writings on the Zapatistas of Chiapas," *Journal of Iberian and Latin American Studies* 3 (December 1997): 109–23; and David LaFrance, "Chiapas in Rebellion: An Early Assessment," *Mexican Studies/Estudios Mexicanos* 12 (Winter 1996): 91–105. The first studies include George A. Collier with Elizabeth Lowery Quaratiello, *Basta! Land and the Zapatista Rebellion in Chiapas* (San Francisco, 1994); Neil Harvey, *Rebellion in Chiapas: Rural Reforms, Campesino Radicalism, and the Limits to Salinismo* (San Diego, Calif., 1994); and Harvey, *The Chiapas Rebellion: The Struggle for Land and Democracy* (Durham, N.C., 1998); John Ross, *Rebellion from the Roots: Indian Uprising in Chiapas* (Monroe, Me., 1995); Carlos Tello Díaz, *La rebelión de las Cañadas* (Mexico City, 1995); and John Womack, Jr., "Chiapas, the Bishop of San Cristóbal, and the Zapatista Revolt," in Womack, ed., *Rebellion in Chiapas: An Historical Reader* (New York, 1999), 3–59.

²⁶ "The state of Chiapas remains akin to a separate country." Lynn Stephen, "Election Day in Chiapas: A Low-Intensity War," *NACLA Report on the Americas* 31 (September–October 1997): 10.

farmers, *campesinos*, whose toil and taxes have for centuries afforded the Ladinos (non-Indians or mestizos) of San Cristóbal a comfortable living.²⁷ They have been a conquered people since the lieutenants of Hernán Cortés arrived in the 1520s and distributed the labor and tribute of their towns as spoils of war. "Into this sheepfold, into this land of meek outcasts," wrote San Cristóbal's namesake, the first bishop of Chiapas, Bartolomé de las Casas, "there came some Spaniards who immediately behaved like ravening wild beasts, wolves, tigers, or lions that had been starved for many days."²⁸

The more than one million *indígenas* in Chiapas are not an undifferentiated mass of "Indians."²⁹ In the highlands, they are organized geographically into numerous municipalities located in different mountain valleys, which structure government and identity. The people of Chamula, Zinacantán, Tenejapa, and the other municipalities generally work, pray, and celebrate together and dress alike. Their traditional intense localism is reinforced by politics and language as well as geography. Native political bosses and their state government collaborators have long promoted community autonomy in the name of "timeless tradition" and thus the political fragmentation of the indigenous highlands. Language differences also divide the native peoples of Chiapas. Tzotzil Maya is spoken in the western highland municipalities, while Tzeltal Maya is spoken in the eastern ones and further east into the lowlands. Zoque, the only non-Maya language in Chiapas, is spoken in the northwest region of the state, while Chol Maya is heard in the northeast near Tabasco, and Tojolabal Maya in the southeast toward the border of Guatemala. Within their communities, Indians are divided by land and wealth (those who have it and those who do not), by religion (traditionalist Catholics, liberation theology Catholics, and evangelical Protestants), and, of course, by politics (the "ins" against the "outs"). The exodus from the highlands to the Selva Lacandona (the last sizable portion of tropical forest in Mexico) in recent decades is breaking down these political and linguistic divisions.³⁰

Not all *kaxlanes* (as Spaniards were and Mexicans are called by the original residents) were rapacious and cruel.³¹ Bishop Las Casas attempted to protect natives from his countrymen. He defined and defended their human rights and

²⁷ Salvador Guerrero Chipres, "94 Municipios de Chiapas de Muy Alta y Alta Marginalidad," *La jornada* (January 3, 1994): 11. San Cristóbal had a population of approximately 90,000 in 1990.

²⁸ Passage from Bartolomé de las Casas, *Brevísima relación de la destrucción de las Indias occidentales*, Herma Briffault, trans., in *Akwe:kon: A Journal of Indigenous Issues* 11 (Summer 1994): 16. The city is described by Henning Siverts, *Oxchuc: Una tribu maya de México* (Mexico City, 1969), chap. 4; also see Thomas Benjamin, "San Cristóbal de las Casas," *Encyclopedia of Latin American History and Culture*, Barbara A. Tenenbaum, ed. (New York, 1996), 5: 31.

²⁹ Since there is no consensus regarding what an "Indian" is, the estimates of population size vary considerably. The 1990 census recorded 716,012 "indígenas," those who speak an Indian language. This figure does not include children under the age of five and adults who speak only Spanish. A more accurate figure could be as high as double the official count. The population of the state of Chiapas in 1990 was 3.2 million. *Indicadores socioeconómicos de los pueblos indígenas de México* (Mexico City, 1990); *Estadísticas básicas de los altos de Chiapas* (Mexico City, 1991).

³⁰ Evon Z. Vogt, "Chiapas Highlands," Robert M. Laughlin, "The Tzotzil," Alfonso Villa Rojas, "The Tzeltal," Roberta Montagu, "The Tojolabal," and Villa Rojas, "Maya Lowlands: The Chontal, Chol, and Kekchi," all in *Handbook of Middle American Indians: Vols. 7-8, Ethnology, Part One*, Vogt, ed. (Austin, Tex., 1969), 133-243.

³¹ Also written as "cashlan" and "caxtlán," "kaxlanes" is a modern survival of *caxtilan*, the Náhuatl word for *castellano* (Castilian), a person from Castile. Natalio Hernández, "Imágenes de los indígenas," *La jornada semanal*, June 30, 1996.



Father Bartolomé de las Casas. A fragment of the mural in Sna Jolobil, in San Cristóbal de las Casas. Photograph by Sharon Lee House, July 1994.

publicized Spanish violations and abuses. No name is more closely associated with the Spanish struggle for justice and compassion for the native peoples of the Americas. San Cristóbal's appropriation of his name in 1848 lent honor to the city that was largely undeserved. The city has raised two monuments to the "Defender of the Indians" and remembers him on the proper anniversaries.³² It has only been since 1960, however, that the spirit of Las Casas has been honored in the city in modern times by anything other than words. It was that year that Samuel Ruiz García was consecrated bishop of San Cristóbal. For more than thirty years, Tatik Samuel (as he is called by his indigenous parishioners, "Dear Father Samuel") has worked to defend the culture and human rights of native peoples. "In this diocese," he declared in a 1994 interview, "we serve the poor, who make up 80 percent of the population. We have to be on the side of those who are suffering the most."³³ It is, in fact, Bishop Samuel who began the chain of events described in this essay.

The five hundredth birthday of Bartolomé de las Casas was celebrated in 1974. The national government of President Luis Echeverría and the state government of

³² The first monument, a life-size statue on a 25-foot base, was erected in 1909. The second monument, a 200-foot plus stone tower capped by a statue, was built in 1974.

³³ Hermosillo, "Interview: Bishop Samuel Ruiz García," 71–72. The best biography of Bishop Ruiz is by Carlos Fazio, *Samuel Ruiz: El caminante* (Mexico City, 1994); also see Gary MacEoin, *The People's Church: Bishop Samuel Ruiz of Mexico and Why He Matters* (Washington, D.C., 1996); and Thomas Benjamin, "Samuel Ruiz García," *Encyclopedia of Mexico: History, Society and Culture*, Michael S. Werner, ed. (Chicago, 1997), 2: 1291.

Chiapas wished to celebrate the occasion and thus publicly affirm their solidarity with Mexico's Indians and the people's cause in history.³⁴ The Indian agencies of the state and national governments had in mind a colorful gathering in San Cristóbal of politicians, academics, and Indian artisans and musicians who would promote tourism and confine questions of human rights to tedious discussions of history. And the events of mid-October at first appeared to fit that bill. A new monument to Las Casas located at the entrance to the city was erected and dedicated. An international panel of jurists discussed Las Casas and human rights. "Lascasian" historians from various American countries north, central, and south considered the honoree's place in history. Marimba bands from the Chiapas highlands gave the events a true festive atmosphere.³⁵

The affair also included an Indigenous Congress, which was sponsored by Bishop Ruiz at the request of the governor. The gathering had been carefully organized from the grass roots months in advance by the bishop's pastoral team. On Columbus Day (in Mexico, it is celebrated as Día de la Raza or Day of the Mestizo Race), approximately 1,200 delegates representing more than 300 native communities met in San Cristóbal. The delegates were Tzeltales, Tzotziles, Tojolabales, and Choles, uniting for the first time spokesmen from the four most important linguistic indigenous populations in Chiapas. Many had previous ties to the church as catechists and translators. They came to discuss in their own languages four themes of vital importance: land, commerce, education, and health. The goal of the congress was to provide a forum for the critical concerns of the indigenous people of Chiapas. As it would be remembered, the Indians found their voice.³⁶

That voice was brutally honest. For three days, Indian delegates described the unhappy reality of Chiapas. Their greatest complaint was the insufficiency of good land. That was the primary cause of their hunger, misery, and exploitation. The agrarian reform process was decades in arrears due to corruption and illegal noncompliance. Ranchers employed gunmen to ensure that poor people did not cultivate part of their pasture lands. Land-poor Indian communities and *ejidos* (land-grant communities) still provided cheap, often child, labor to commercial plantations. Business in the highlands was controlled by Ladino and Indian *caciques* (bosses) who, allied with government agencies, bought their produce for little and sold them goods for a lot. Education and health care hardly existed, as high rates of illiteracy and infant mortality demonstrated. Where schools and clinics had been built, they were rarely staffed by teachers, nurses, and doctors or provided with books and medicines. Chronic alcoholism made every problem worse.³⁷

³⁴ In 1992, as in 1974, commemoration of Las Casas was an occasion to declare that "the Government of Chiapas has made common cause with the indigenous people." See *Unidad: Camino de reconciliación y esperanza; Homenaje a Fray Bartolomé de las Casas* (San Cristóbal de las Casas, 1992), 63.

³⁵ Fazio, *Samuel Ruiz*, 103–04; Juan Ojeda, "El Congreso Indígena," *Caminante* 45 (Diocese of San Cristóbal de las Casas, October 1988); "La vuelta del Katún," *El perfil de La jornada*, October 12, 1994.

³⁶ Jesús Morales Bermúdez, "El Congreso Indígena de Chiapas: Un testimonio," *América indígena* 55 (January–June 1995): 311; Antonio García de León, "La vuelta del Katún," *El perfil de La jornada*, October 12, 1994; Ana Bella Pérez Castro, "Apéndice 2: El Congreso Indígena," in *Entre montañas y cafetales* (Mexico City, 1989), 189–90.

³⁷ Francis Mestries, "Testimonios del Congreso Indígena de San Cristóbal de las Casas, octubre de 1974," in Pilar López Sierra, et al., eds., *Historia de la cuestión agraria mexicana*, 9: *Los tiempos de la crisis 1970–1982* (Mexico City, 1990), 473–89; Juan González Esponda, "El Congreso Indígena de 1974:

Never before had Tzotziles, Tzeltales, Tojolabales, and Choles spoken with one another like this, listened to others with similar problems, or viewed themselves as one people (not unlike their possible role model, the Israelites of the Exodus, a people enslaved but promised liberation by their Moses). It was a revelation. Giving voice to one's problems is a requisite to solving them.³⁸ "Where, then," asked one delegate, "where is the liberty Brother Bartolomé left us? Well, *compañeros*, Brother Bartolomé is no longer alive. We have made this Congress in his name, he is dead and we can't expect another. Who will defend us against injustice and give us liberty? I don't believe the Ladinos will defend us. The government, perhaps will or perhaps will not. Therefore, who will defend us? I believe that all of us organized together can have liberty and can work better. All of us together can be Bartolomé."³⁹

The congress tried to make itself a permanent institutional force in Chiapas. A president, secretary-general, and four regional coordinators were elected and met periodically until March 1977. The leaders and participants of the congress became missionaries of inter-community and inter-ethnic activism and organization when they returned to their communities. They became the founders of new producer, transport, and consumer cooperatives to help *campesinos* lower their costs and increase their earnings. They brought in specialists to teach individuals in many communities how to treat simple medical problems and prevent more serious maladies. They formed alternative bilingual schools. They encouraged the translation of political, agricultural, and historical texts into Tzotzil, Tzeltal, Tojolabal, and Chol and the writing of their own stories and histories in their own languages.⁴⁰

Congress participants from the new Tzeltal communities in the Lacandón frontier also organized confederations of *ejidos*. The object was to come together in the bureaucratic and political struggle to reactivate land reform. "Improving Our Lives Through Our Collective Force," "Quiptic ta Lecubtesel" in Tzeltal, formed originally by eighteen *ejidos* in the San Quintín region, was legally constituted in December 1975. "Tierra y Libertad" (Land and Liberty) and "Lucha Campesina" (Peasant Struggle) in the Las Margaritas region were organized soon thereafter. In 1980, an alliance of 180 *ejido* communities, the Union of Community Unions, was

Contexto y consecuencias," *Memorias del Primer Congreso Internacional de Mayistas*, 165–82; and Mestries, "Primer Congreso Indígena," *Cultura y sociedad* 1 (1974). Selections from the congress have been translated into English in "Las Casas Recalled, Indians Informed, Organized, United, and Defiant: The Congress of San Cristóbal, 1974," in Womack, *Rebellion in Chiapas*, 148–61. *Ejidos* are land-grant communities that collectively possess the nation's land in usufruct.

³⁸ Exodus is a very important element of religious instruction in the indigenous highlands and in the Lacandón forest. The book of Exodus was the first book of the Bible translated into Tzeltal in 1972–1974. The catechism lesson book in a bilingual Spanish-Tzeltal edition, *Estamos buscando la libertad*, directly compared the Indian migration into the Lacandón to the biblical Exodus. Enrique Maza, "Juntas, la acción política y la acción pastoral concientizaron a los indígenas en la búsqueda de su redención," *Proceso* (February 7, 1994): 22–25; Michael Tangeman, *Mexico at the Crossroads: Politics, the Church, and the Poor* (Maryknoll, N.Y., 1995), 8–9; Michael Walzer notes that the story of Exodus has for centuries been read as a metaphor for revolution and liberation, in his captivating *Exodus and Revolution* (New York, 1985).

³⁹ Mestries, "Testimonios del Congreso Indígena," 475.

⁴⁰ Ana Bella Pérez Castro, "Movimiento campesino en Simojovel, Chis. 1936–1978: Problemas étnico o de clases sociales," *Anales de antropología* 19 (1982): 207–29; Luis Méndez Asensio and Antonio Cano Gimeno, *La guerra contra tiempo: Viaje a la selvaalzada* (Mexico City, 1994), 147–63.

forged under Quiptic's leadership. During the late 1970s and throughout the 1980s, *campesino* organizations sprouted in all regions of Chiapas. There was, as one observer described it, "an explosion of peoples' organizations."⁴¹ The hope expressed by one Tzotzil speaker at the congress in 1974 was becoming reality: "the force of the communities will become the new Bartolomé."⁴²

The veterans of the Indigenous Congress of 1974 with the assistance of a politicized clergy and transplanted political activists from other parts of Mexico began to radicalize politics in Chiapas at the grass roots. This was not what the government had in mind, of course, when it asked the bishop to sponsor an indigenous congress. In an effort to control or at least divide the emerging indigenous movement in Chiapas and throughout Mexico, the government's National Peasant Federation (Confederación Nacional Campesina, CNC) organized its own Congress of Indigenous People, held in Pátzcuaro Michoacán, in 1975. From its sessions emerged the National Council of Indian Peoples, which was designed to coopt growing native radicalism.⁴³ In Chiapas, in the Tzeltal region, the like-minded Cooperative Society of Coffee Growers was formed to win pro-government supporters. Since the CNC had vast government resources to grant, and had influence with the agrarian reform bureaucracy, it gained support and produced a schism within the Union of Community Unions.⁴⁴

There were also new divisions within communities. The Indigenous Congress of 1974 emboldened a faction of dissidents in Chamula to oppose the corrupt alliance between indigenous municipal leaders (who also controlled land allocations and the liquor concession) and state and national authorities.⁴⁵ The dissidents, branded as "evangelical Protestants" and thus pariahs, were deprived of their lands and burned out of their homes. They had nowhere to settle but the impoverished peripheral neighborhoods of San Cristóbal, the *cinturones de miseria* or belts of misery. This happened in many highland communities as the years passed, and some tens of thousands of dispossessed Indians created squatter settlements around the city. For the first time, highland Indians began to live and work in San Cristóbal in large numbers as Indians rather than as assimilated mestizos. State authorities turned a blind eye to these dispossessions, declaring that indigenous communities were autonomous and had a right to defend their culture, customs, and traditions.⁴⁶

⁴¹ The phrase is Ernesto Reyes's, quoted in Thomas Benjamin, *A Rich Land, a Poor People: Politics and Society in Modern Chiapas*, 2d edn. (Albuquerque, N.Mex., 1996), see the epilogue and 235–45 for more detail.

⁴² Morales Bermúdez, "El Congreso Indígena," 317; Neil Harvey, "La Unión de uniones de Chiapas," *La jornada del campo* (October 13, 1992): 10.

⁴³ In time, the National Council of Indian Peoples became more independent of the government and began to criticize presidential actions. This led to its dissolution by President José López Portillo in 1980. Alexander Ewen, "Mexico: The Crisis of Identity," *Akwe:kon Journal* 11 (Summer 1994): 34.

⁴⁴ Luis Hernández Navarro, "Chiapas: Del Congreso Indígena a la guerra campesina," *La jornada del campo* 23 (October 25, 1994): 1–3; Neil Harvey, "Estrategias corporativistas y respuestas populares en el México rural: Estado y organizaciones campesinas en Chiapas desde 1970," *CHIMECH* 2 (August 1991): 60–61.

⁴⁵ The origin of this system is explained by Rus, "'Comunidad Revolucionaria Institucional.'"

⁴⁶ This outrage was first exposed in the pamphlet by Juan Jaime Manguén, *et al.*, *La violencia en Chamula* (San Cristóbal de las Casas, 1978). Also see Gaspar Morquecho Escamilla, "Expulsiones en los altos de Chiapas," in *Movimiento campesino en Chiapas* (San Cristóbal de las Casas, 1994), 63; Oliver Tickell, "Indigenous Expulsions in the Highlands of Chiapas," *International Work Group on Indigenous Affairs Newsletter* 2 (1991): 9–14; "Expulsiones Indígenas y el respeto a las culturas,

"The Indian Congress," writes historian John Womack, Jr., "stunned conservative San Cristóbal."⁴⁷ Its multiple effects reverberated throughout Chiapas and the highlands. The prospect of indigenous revitalization had never been viewed kindly by this city, not in 1712 during the Great Tzeltal Rebellion nor during the so-called "Caste War" of 1869.⁴⁸ It was not welcomed during the "Revolution of the Indians" in the 1930s—land, labor, and educational reforms that were imposed by the national government and partially reversed after 1946 by conservative municipal and state governments whose goal was to "subdue" the natives.⁴⁹ In short, writes photographer Antonio Turok, "the memory of past rebellions has kept the mestizos terrified of losing dominance."⁵⁰ In the 1970s, once again the Indian "hordes" were at the gates of the city. The threat—in the form of diminishing deference, increasing residence in the city, and, most serious of all, growing Indian solidarity across community boundaries—was palpable.⁵¹ Some spoke of the "Indianization" of San Cristóbal and a more widespread "uprising" throughout Chiapas. The citizens of San Cristóbal feared they were losing control of the highlands and their city.⁵²

FOUR YEARS AFTER THE INDIGENOUS CONGRESS, the municipal government of San Cristóbal de las Casas organized an impressive commemoration of the founding of the city on its 450th anniversary. Municipal President José Jiménez Paniagua, a descendant of one of the oldest and most prominent families in San Cristóbal, directed the affair and obtained the participation of the mayor of Ciudad Real, San Cristóbal's sister city in Spain. The celebration began on October 12, 1977, and concluded in April the following year.

The commemoration focused on the year 1528 and the man of the year, Captain Diego de Mazariegos. Planners emphasized that it was not a celebration of the conquest of Chiapas, which had occurred earlier, in 1524, by an expedition headed by Luis Marín. Mazariegos was dispatched three years later to suppress an Indian rebellion and definitively settle the province. He entered the region in March 1528

costumbres y tradiciones de esos pueblos en Chiapas," *Anuario indigenista* 31 (December 1992): 337–89.

⁴⁷ Womack, "Bishop's Conversation," 32. "The Indigenous Congress of October 1974 is the obligatory reference point for understanding and explaining the organization and struggle of the indigenous campesinos of Chiapas." María del Carmen Legorreta Díaz, "Política y guerrilla," *Nexos*, January 1997.

⁴⁸ "Ladinos and Indians fear one another. Ladinos are afraid of Indian vengeance." Siverts, *Oxchuc*, 48.

⁴⁹ Rus, "'Comunidad Revolucionaria Institucional,'" 257–64.

⁵⁰ Antonio Turok, "Chiapas: The End of Silence," in Turok and Francisco Alvarez Quiñones, *Chiapas: El fin del silencio/The End of Silence* (Mexico City, 1998), 25.

⁵¹ María del Carmen Legorreta Díaz refers to "the fear of a 'caste war' in the air of Ladino cities." "Chiapas," in Pablo González Casanova and Jorge Cadena Roa, eds., *La República Mexicana: Modernización y democracia de Aguascalientes a Zacatecas*, vol. 1 (Mexico City, 1994), 126.

⁵² Interview with Andrés Aubry, director of the historical archive of the Cathedral of San Cristóbal and founder of the Instituto de Asesoría Antropológica para la Región Maya, A.C., in San Cristóbal, July 1994. During 1978–1979, I frequently visited Dr. Prudencio Moscoso Pastrana—the official chronicler of the city—at his home in San Cristóbal to use his library. He held mid-afternoon tea-and-coffee discussions with friends and colleagues and occasionally invited me. There I listened to city residents (*coletos*) talk about the issues of the day including "the Indian problem."

and established a Spanish town, Villa Real de Chiapa de los Españoles, in the lowlands on the banks of the Grijalva River, near the largest Indian town, the Chiapanec capital of Socton Nandalumi. Less than a month later, he transferred the new Spanish settlement to the colder and healthier highlands in a valley called Hueyzacatlán (in Nahuatl) and Jovel by the local native people. The site was ideal: it was more centrally located in the province, the climate was temperate, there was plenty of land for pasture and farming, and—according to Mazariegos—it was not already occupied by natives. This was the official story of the kinder and gentler conquistador and the peaceful founding of Villa Real, soon to be Ciudad Real, and today known as San Cristóbal.⁵³

This story has a long and distinguished pedigree. It arose hundreds of years earlier to counter the legend of Bishop Las Casas's curse on Ciudad Real for its exploitation of Indians. In 1619, the Dominican friar Antonio de Remesal produced the first chronicle of the founding of Chiapas.⁵⁴ He argued that Ciudad Real's blackened reputation as the enemy of one of the great humanitarians of history was undeserved. Yes, there were some Spaniards who exploited Indians and criticized Las Casas, but the city itself should not be condemned. Indeed, Remesal noted, Ciudad Real's name was forever linked not simply to one hero but to two. The "Protector of the Indians" was preceded by the peaceful conqueror, Diego de Mazariegos—a chivalrous gentleman in an age of rapacious soldiers of fortune. He was, according to the chronicler, "the patron and protector of the *naturales*" and "very humane in his good treatment of the Indians." Thereafter, local historiography faithfully followed Remesal's lead. Mariano Robles Domínguez de Mazariegos in his 1813 history of the province praised Captain Mazariegos for "achieving the pacification [of Chiapas] without recourse to force." Vicente Pineda, writing in 1888, repeated the story of peaceful conquest, while Manuel B. Trens in 1957 praised Mazariegos for founding a town "without prejudice to the *naturales*." Mazariegos, Francisco Santiago Cruz wrote in 1974, "made a considerable effort to pacify the Indians by love."⁵⁵

Modern scholars agree that Captain Mazariegos achieved the definitive conquest and domination of the province in the months following the founding of Villa Real.⁵⁶ The historical record of the events of the late 1520s and early 1530s is incomplete and contradictory, but there are scattered references to Indian resis-

⁵³ E. Flores Ruiz, *Libro de oro de San Cristóbal de las Casas* (San Cristóbal, 1976). This local historian wrote the historical narrative for the official publications of the 1978 commemoration. A description of the region on the eve of the Spanish conquest is given by Jan de Vos, "Chiapas en el momento de la conquista," *Arqueología mexicana* 2 (June–July 1994): 14–21. Ciudad Real was known informally as San Cristóbal de los Llanos. The valley of San Cristóbal contains numerous archeological sites pertaining to the ancient Maya and was fully occupied in 1524, according to accounts of that first expedition. The issue of Maya occupation in 1528 is bitterly contested today.

⁵⁴ Antonio de Remesal, *Historia general de las Indias Occidentales, y particular de la gobernación de Chiapas y Guatemala* (1619), 2 vols. (Guatemala, 1932).

⁵⁵ Francisco Santiago Cruz, *Ciudad Real de Chiapas en la historia de Fray Antonio de Remesal* (Tuxtla Gutiérrez, 1974); and *San Cristóbal de las Casas en el relato de sus historiadores* (Mexico City, 1981). Remesal's chronicle and influence is the subject of an excellent analysis by Jan de Vos in *Los enredos de Remesal: Ensayo sobre la conquista de Chiapas* (Mexico City, 1992). Vicente Pineda, *Historia de las sublevaciones indígenas habidas en el estado de Chiapas* (Chiapas, 1888). The Remesal tradition continues today with José Antonio Gutiérrez, *Infundios contra San Cristóbal de las Casas* (Mexico City, 1996).

⁵⁶ Bricker, *Indian Christ, the Indian King*, chap. 4, 43–52; Jan de Vos, *Vivir en frontera: La experiencia*

tance to the Spanish expedition in 1528, the deportation of Indian slaves as spoils of war, and a battle wound received by Mazariegos himself. The royal certificate granting city status and a coat of arms to what was Ciudad Real in 1535 referred to the "great sacrifices" the conquerors made to subdue the province. It is unclear whether the captain was present at the battle of Sumidero in 1532, when, according to legend, Chiapaneca rebels in an act of heroic defiance threw themselves into the steep Sumidero Canyon rather than accept Spanish conquest and slavery.⁵⁷

The conquest of Chiapas was unquestionably violent and tragic for the indigenous peoples of the province. Three terrible maladies struck the natives in the decades immediately following: disease, slavery, and tribute. A measles epidemic swept through the towns of the Chiapanecas as early as 1529. Within fifty years, periodic pandemics of pneumonia, smallpox, and bubonic plague reduced the total indigenous population by two-thirds.⁵⁸ Indian slavery became endemic in Chiapas after the conquest. The first town council of Villa Real in 1528 enacted a regulation that stated, "regarding those Indians who refuse to give provisions to the Spaniards, war shall be declared, and those who are taken prisoner shall become slaves." The conquerors-turned-*encomenderos* (tribute collectors) built sugar plantations in the lowlands and faced a labor shortage due to the effects of disease. When Bishop Las Casas arrived in the province in 1545, he reported that "the great number of slaves they made is incredible." The *encomenderos* also had the right to collect tribute from their subject towns. When one royal investigator arrived in Ciudad Real in 1548, he found that free Indians suffered under such onerous tributes that their condition was little better than slavery.⁵⁹

The brief tenure of Bishop Las Casas in Chiapas (1545–1547) was marked by constant conflict with the local *encomenderos* and settlers of Ciudad Real. Las Casas and his Dominican friars settled in Indian villages, worked to enforce the 1542 royal decree abolishing Indian slavery (with little success) and encouraged natives to resist demands for excessive tribute and labor. The *encomenderos*, in turn, characterized Las Casas as that "antichrist of a bishop," blamed the clerics for the labor shortage and poor royal revenues, and demanded a free hand in their control and treatment of the natives. Las Casas left Chiapas to pursue his cause at the royal court, and in time the local clergy became an indistinguishable part of the provincial elite also subsisting on native tribute and labor.⁶⁰

de los indios de Chiapas (Mexico City, 1994), 95–96; Gudrun Lenkersdorf, *Génesis histórica de Chiapas, 1522–1532: El conflicto entre Portocarrero y Mazariegos* (Mexico City, 1993).

⁵⁷ Juan M. Morales Avendaño, "Tópicos históricos de la época de la conquista y colonial de Chiapas," *Segundo encuentro de intelectuales Chiapas-Centoamerica* (Tuxtla Gutiérrez, 1992), 265–70; Gudrun Lenkersdorf, "La resistencia a la Conquista Española en los altos de Chiapas," in Juan Pedro Viqueira and Mario Humberto Ruz, eds., *Chiapas: Los rumbos de otra historia* (Mexico City, 1995), 78–82; Mónica del Villar K., "La leyenda del Sumidero," *Arqueología mexicana* 2 (June–July 1994): 32–35; and Jan de Vos, *The Battle of Sumidero: A History of the Chiapanecan Rebellion through Spanish and Indian Testimonies (1524–34)* (Amsterdam, 1996), 9–25.

⁵⁸ Peter Gerhard, *The Southeast Frontier of New Spain* (Princeton, N.J., 1979), chap. 4.

⁵⁹ William L. Sherman, *Forced Native Labor in Sixteenth-Century Central America* (Lincoln, Neb., 1979), 149; and Nélida Bonaccorsi, *El trabajo obligatorio indígena en Chiapas, siglo XVI* (Mexico City, 1990). Also see Murdo J. MacLeod's excellent *Spanish Central America: A Socioeconomic History, 1520–1720* (Berkeley, Calif., 1973). *Encomenderos* were granted the authority to collect tribute from specified native communities.

⁶⁰ Wasserstrom, *Class and Society in Central Chiapas*, 16–26.

The subsequent development of Ciudad Real/San Cristóbal was the story of government and a few primary families extracting wealth from the surrounding indigenous population by various and sometimes very imaginative means. In the seventeenth century, the instrument of choice was the *repartimiento de mercancías*, a system of forced sales by which Indian communities were compelled to trade foodstuffs, raw cotton, tobacco, and cacao beans for expensive finished goods. In 1712, this abuse pushed natives to the edge of starvation and sparked the Tzeltal uprising, one of the bloodiest Indian rebellions in colonial Mesoamerica.⁶¹ In the nineteenth century, Cristobalense elites substantially expanded their haciendas and expropriated native land holdings. This turned Indians into renters, sharecroppers, debt peons, and migrant laborers and—periodically—desperate, messianic rebels. In the 1870s, Chiapas was characterized by the Mexico City press as the “slave state” of Mexico. A reform-minded Chiapas state governor in 1896 criticized the oligarchy of San Cristóbal for “squeezing the juice out of [Indians], maintaining them in servitude for a peso a month, sucking their blood like voracious vampires in all kinds of little contracts.” Even after the Mexican Revolution, little had changed. The national Department of Indigenous Affairs reported in 1936 that “conditions of virtual slavery exist in Chiapas.”⁶² A visitor in 1942 was told that the city “lives from the labor of the Indians, principally that of the Chamulas.”⁶³

During the postwar era, the national government implemented extensive land reform in the indigenous highlands and built roads and schools to integrate and assimilate Indians into Mexican society. The government and ruling party also created and installed an Indian political oligarchy in each municipality, known collectively as *caciques*, which still control access to land, commerce, and liquor. *Caciques*, in collaboration with the ruling party, closed their communities to outside “interference” and thus facilitated government control over the majority albeit divided Indian population of the highlands. Native communities remained extremely poor. Commercial Ladino farmers and planters throughout the state took advantage of cheap Indian labor to clear their fields, plant their crops, and pick their coffee. Indians still complained that “in Chiapas the *finqueros* treat Indians worse than their animals.”⁶⁴ San Cristóbal, which for a century had been losing control of Indian labor and production, was left a poor but proud backwater city. The *coletos*—as residents of San Cristóbal call themselves—still managed to exploit the surrounding population in marginal ways and emphasize their superiority in petty ways, such as requiring Indians to cross their arms and bow submissively, as well as walk in the cobblestone streets, reserving the sidewalks for Ladinos.⁶⁵

⁶¹ Kevin Gosner, *Soldiers of the Virgin: The Moral Economy of a Colonial Maya Rebellion* (Tucson, Ariz., 1992), chap. 6.

⁶² Quotations from Benjamin, *Rich Land, a Poor People*, 28, 67–68, 191. Also see Jan De Vos, “Ser indio en Chiapas,” *Siglo XX* 15 (January–June 1994): 131–60.

⁶³ Eulalia Guzmán, “Un viaje a San Cristóbal de las Casas,” *Antropológicas* 10 (1994): 79. Two decades before, Frans Blom and Oliver LaFarge noted: “Economically San Cristóbal cannot exist without the Indians.” Quoted in Aguirre Beltrán, *et al.*, *El indigenismo en acción: XXV aniversario del Centro Coordinador Indigenista Tzeltal-Tzotzil, Chiapas* (Mexico City, 1976), 13.

⁶⁴ From the statement of a Chol of Palenque to Governor Patrocinio González Garrido in 1992. Araceli Burguete Cal y Mayor, “Las cuentas pendientes,” *Memoria* 63 (February 1994): 33. A *finquero* is an owner of a landed estate, or *finca*.

⁶⁵ Benjamin N. Colby and Pierre L. Van Den Berghe, “Ethnic Relations in Southeastern Mexico,” *American Anthropologist* 63 (August 1961): 772–91; and Colby, *Ethnic Relations in the Chiapas*

This was the San Cristóbal de las Casas that faced the challenges of the 1970s with such trepidation. Having lost its real power and authority long before and facing an assertive agrarian movement and a wave of Indian migrants, the city was left with little more than symbolic means to vent its frustrations, combat its fears, and express its superiority. The commemorative events of 1978, and particularly the raising of a monument to Mazariegos, were therefore significant. "In San Cristóbal," writes one student of the city, "Ciudad Real was reviving."⁶⁶

On March 27, San Cristóbal welcomed the mayor of Ciudad Real, Spain—the hometown of Diego de Mazariegos—as the guest of honor during the "Day of Cristobalense Spanishness." A military parade with 3,000 soldiers followed speeches that praised the "civilizing labor" of Spanish colonization and the idealism of the founder Mazariegos, "a man of la Mancha." During the following days, the city sponsored sports events, a beauty contest, dances and musical performances, bullfights, the inauguration of public works, a fireworks display, ceremonies at both monuments to Las Casas, a round-table discussion of current social problems in Chiapas, an award ceremony, official receptions, speeches and more speeches. The governor of Chiapas participated in several of the events, and the president of Mexico arrived for the closing ceremonies on April 4.⁶⁷

The highlight of the commemoration was the unveiling of the "Monument to the Founder of the City Diego de Mazariegos" on March 31. The monument itself was a slightly larger than life-size bronze statue of an armed and armored sixteenth-century soldier standing on a concrete base. This was unmistakably the classic image of a conquistador. Although invited representatives of highland native communities were present at the ceremony and the governor's speech praised the peaceful coexistence of the two races, the symbolism of the monument was clearly insolent in modern Mexico. The people of San Cristóbal understood this. One month before the unveiling, the former mayor of the city, Leopoldo Velasco Robles, commented, "it is rather audacious to erect a monument to a conquistador since he as well as his colleagues did not practice the most humanitarian methods to enlighten the native."⁶⁸ Unknown vandals understood it as well, apparently, when they stole the conqueror's sword one day after the ceremony and a second time a week later, after the city replaced it. President José López Portillo, a talented politician in tune with the requirements of populist nationalism, also understood the symbolism and steered clear of the Mazariegos monument during his visit to the city. The president instead made a pilgrimage to the newer monument to

Highlands of Mexico. The term *coleta*—pigtail or hank—is from the eighteenth century and refers to the hairstyle traditionally worn by bullfighters.

⁶⁶ "In 1978, no less than four years after the first massive expulsion of Chamulas opposed to the PRI, a statue to the conquistador Diego de Mazariegos was erected, in front of the principal entrance of the temple of Santo Domingo." Magdalena Patricia Sánchez Flores, "De la ciudad real a la ciudad escaparate," in Diana Guillén, ed., *Chiapas: Una modernidad inconclusa* (Mexico City, 1995), 82, 106.

⁶⁷ *Memoria 450 Aniversario: 1528–1978* (San Cristóbal de las Casas, 1978). This booklet was the official guide and report of the commemorative events. My understanding of the commemoration is also based on an interview with Lic. José Jiménez Paniagua, San Cristóbal's municipal president in 1977–1979, conducted in San Cristóbal, July 1994. The *licenciado* kindly gave me a copy of the official guide.

⁶⁸ Matilde Pérez U., "Polémica por la destrucción de la estatua de Diego de Mazariegos," *La jornada*, October 14, 1992.



Governor Salomón González Blanco unveils the monument to Diego de Mazariegos, March 31, 1978. From *Memoria 450 Aniversario: 1528–1978* (San Cristóbal de las Casas, 1978).

Las Casas, where he laid a floral wreath and praised San Cristóbal's association "with one of the men who redeemed the dignity of humanity in the sixteenth century."⁶⁹

Mainstream Mexican culture has long repudiated the deeds of its Spanish conquerors and consigned those antiheroes to historical oblivion. One of the oldest and most important commemorative monuments in Mexico City is the 1887 statue of the last Aztec emperor, Cuauhtémoc, who was defeated, tortured, and executed by the conquistador Hernán Cortés. As for Cortés, writes Enrique Krauze, "no street, no statue, no city dares to call itself by his damnable name."⁷⁰ It was in this context that San Cristóbal glorified its founder. "It constitutes the only monument in honor of a conquistador to be found in all of the republic."⁷¹

⁶⁹ "La cultura es la consumación y justificación de la democracia," *El universal* (Mexico City), April 4, 1978. After the second theft, the Mazariegos statue remained unarmed until its demise in 1992.

⁷⁰ Enrique Krauze, "Founding Fathers," *New Republic* (November 28, 1994): 66.

⁷¹ De Vos, *Los enredos de Remesal*, 47.

IN THE SPRING OF 1992, A GROUP OF about 400 Indians from Chiapas arrived in Nezahualcoyotl, one of the world's largest slums, on the outskirts of Mexico City. For six weeks, they had walked from Chiapas on a protest march they called *xi'nich* in Chol and "Hormiga que Marcha" (March of the Ants) in Spanish. Their protest was spurred by the violent eviction of members of the Committee of Defense of Indigenous Liberty from the Mayan ruins and popular tourist site of Palenque located in northern Chiapas. Their march sought to draw national attention to local government corruption and native land claims.⁷² As they entered Nezahualcoyotl, the marchers were greeted by supporters who gave them flowers and a meal. In the crowd was a man holding a sign that read "Welcome to History."⁷³

The road to History for the indigenous peoples of Chiapas has been laden with conflict and struggle.⁷⁴ The "explosion of peoples' organizations" in the 1970s and 1980s was based primarily on agrarian demands. Agrarian mobilization, however, led to an indigenous revitalization movement more broadly based.⁷⁵ New organizations concerned with issues of racism, language, credit, human rights, health care, autonomy, and women's rights appeared. Local indigenous groups found recognition and support in the Plan de Ayala National Coordinating Body (1979). Representatives of more than 280 popular organizations met in San Cristóbal in January and February in 1994 to form the State Council of Indigenous and Peasant Organizations of Chiapas.⁷⁶

The movement, like many other indigenous organizations, developed its greatest strength in the new communities of the Selva Lacandona.⁷⁷ Since the 1950s, more than 60,000 immigrants, most from the indigenous highlands, settled in the lowland valleys and forest, carved small farms out of the bush, and created multi-ethnic and multi-lingual communities—3,000 of them.⁷⁸ Being new and unplanned, these

⁷² The governor of Chiapas stated that the communities from which the marchers came had received in the previous three years "more attention than they deserved." José Chablé and Regina Martínez, "Imposible superar en tres años males de siglos: González Garrido," *La jornada*, April 9, 1992.

⁷³ Hermann Bellinghausen, "Abril de Xi'Nich," *Ojarasca* 8 (May 1992): 13. What did this mean? I can only conjecture that "History" refers to the great flow of events that shape Chiapas, Mexico, and the world.

⁷⁴ Agrarian conflict with landowners and the government accounted for most indigenous "criminality." Ninety percent of the approximately 2,500 prisoners in Chiapas jails in 1994 were Indians. Guillermo Correa, Salvador Corro, and Julio César López, "En las cárceles del estado, prolongación de las fincas, el 90% de los presos son indígenas," *Proceso* (February 21, 1994): 25.

⁷⁵ The broader Latin American indigenous movement began in 1971 at the Barbados conference on the Liberation of the Indian sponsored by the World Council of Churches. The "Barbados Group" met for a second time on the same island in 1977 and a third time in Rio de Janeiro, Brazil, in 1993. The Third Declaration of Barbados states: "Indian peoples have an undeniable right to their history and cultural heritage." See "Declaración de Barbados III," *Ojarasca* 33–34 (June–July 1994): 42. The First and Second International Forums on the Human Rights of Indigenous Peoples were held in Mexico in 1989 and 1990.

⁷⁶ Neil Harvey, "Las organizaciones sociales ante el conflicto armado de Chiapas," *El cotidiano* 61 (March–April 1994): 21–25; Ricardo del Muro, "Movimientos campesinos: La violenta lucha por la tierra," *Macrópolis* (January 31, 1994): 16–19; Hernández Navarro, "Chiapas: Del Congreso Indígena a la guerra campesina," 1–3.

⁷⁷ Joel Simon, *Endangered Mexico: An Environment on the Edge* (San Francisco, 1997), chap. 4, 91–125.

⁷⁸ Approximately 200,000 people lived in the Selva Lacandona in the mid-1990s, compared to about 91,000 in 1980, 40,000 in 1970, and 1,000 in 1950. Lourdes Arizpe S., Fernanda Paz, and Margarita Velázquez, *Cultura y cambio global: Percepciones sociales sobre la deforestación en la Selva Lacandona* (Mexico City, 1993), 69; Jan de Vos, "El Lacandón: Una introducción histórica," in *Chiapas: Los rumbos de otra historia*, 355.

communities were largely free from government control, politically cooperative, and remarkably democratic; they also, however, came under attack from local Ladino ranchers and landowners who had set their sights on the same land. Tzeltal, Tzotzil, Tojolabal, and other Indians made new homes together for the first time. They had to learn each other's languages, they modified their customs to get along, and abandoned some of the ways of their fathers.⁷⁹ In short, they began to identify themselves as "poor *campesinos*," "indigenous people" and Maya rather than Chamulans, Tenejapans, and Cancuceros. "One has to wonder," noted an indigenous scholar, "what has happened to their world view, their self-image."⁸⁰

In the new frontier, the colonists were sought out by left-wing political activists, liberation theology priests and catequists, and Protestant missionaries. There was a "systematic and intense" incursion into the region by leftist students from Mexican universities. Mexican Communist Party activists and militants of the Mao-inspired Proletarian Line movement from northern Mexico came to Chiapas in the 1970s.⁸¹ Thousands of lay catechists encouraged settlers "to speak out, to think about the world. Without a doubt," remarks a local priest, "this has contributed to the indigenous people gaining greater awareness."⁸² The colonists listened to programs in Tzotzil and Tzeltal on the radio. In the new communities and *ejidos*, classes were given in the Bible, agriculture, politics, and revisionist Mexican history. Images of Emiliano Zapata, Che Guevara, and Karl Marx began to replace those of saints in some *ejido* offices and community halls.⁸³

The more politically and culturally conservative indigenous communities in the highlands also experienced considerable change. Political conflict produced tens of thousands of refugees, as dissidents (often labeled as "evangelistas") opposed to local political bosses were expelled from their homes. Often, those who were expelled became Protestants if they were not before. Protestantism did find adherents in the highlands, as individuals and communities, unhappy with alcohol-centered rituals or attracted by personal and community reform, left "the traditional religion and accepted Christ the Lord."⁸⁴ Economic pressures and opportu-

⁷⁹ Anna María Garza Caligaris, et al., *Sk'op Antzetik: Una historia de mujeres en la selva de Chiapas* (Tuxtla Gutiérrez, 1993), 42; Xóchitl Leyva Solano and Gabriel Ascencio Franco, "Lacandonia al Filo del Agua," *Ojarasca* 33-34 (June-July 1994): 9-13.

⁸⁰ Xóchitl Leyva Solano, "Notas sueltas acerca de identidad y colonización: La Selva Lacandona en las postrimerías del siglo XX," in *Segundo encuentro de intelectuales Chiapas-Centroamérica*, 308-14. Mixtecs in similar circumstances far from their homeland are discovering that they are Mixtec. "A new political consciousness and activism has coalesced into an emerging pan-Mixtec ethnic identity, an ethnic awareness that transcends community and even district identification and manifests itself in the form of Mixtec associations and labor-union activity in the border area of the Californias and Sonora and in Oregon." Carole Nagengast and Michael Kearney, "Mixtec Ethnicity: Social Identity, Political Consciousness, and Political Activism," *Latin American Research Review* 25 (1990): 80-81.

⁸¹ María Concepción Obregón R., "La rebelión zapatista en Chiapas: Antecedentes, causas y desarrollo de su primera fase," *Mexican Studies/Estudios Mexicanos* 13 (Winter 1997): 175-76.

⁸² Father Jorge Rafael Díaz Nuñez of Ocosingo, quoted in *Chiapas: Rebellion of the Excluded* (Washington, D.C., 1994), 20.

⁸³ Juan Francisco Medina Gutiérrez writes that Indians in Chiapas were attracted to Marxism. "The world of theory opened before their eyes that which they confronted on a daily basis: exploitation." "La larga lucha por una nación indígena," *Macrópolis* (January 31, 1994): 51.

⁸⁴ Ricardo Pérez P., *Historia de un pueblo evangélico: Triunfo agrarista* (San Cristóbal de las Casas, 1993). One of the new indigenous organizations was composed of "los expulsados," the expelled ones: El Comité de Defensa de los Amenazados, Perseguidos y Expulsados de Chamula in 1984. See María Ester Ibarra, "Los conflictos religiosos," *Macrópolis* 99 (February 7, 1994): 8-19.

nities, increased inequality among neighbors, new roads and more trucks, and a variety of government programs helped produce what anthropologist Frank Cancian has called “the decline of community.”⁸⁵

The indigenous peoples of the Selva Lacandona and the highlands have been subject to ever increasing efforts at “conscientización”—consciousness raising. Christian Base Communities encourage members to “ver, analizar, y actuar” (see, think, act) and apply the New Testament to their lives.⁸⁶ Pastoral agents “have collaborated in the *concientización* of the poor,” writes Father Joel Padrón, a diocesan priest in Simojovel.⁸⁷ Indigenous catechists taught not only the Bible but “courses in native languages on the history of Mexico and Chiapas, political economy, and Mexican commerce.”⁸⁸ Traveling theater groups such as Lo’il Maxil, a Tzeltal-language group, provided entertainment and education. “For the first time in 500 years,” writes Petrona de la Cruz, “we are told about the origin of our history and our ancient religion, we are shown how our dynasties were founded and how our ancestors lived in the classic period of the Maya.”⁸⁹ Histories, stories, and legends in Mam Maya, produced by local people about the subjects that most interested them, began to be broadcast in 1988 by radio XEVFS, in Margaritas, Chiapas, “The Voice of the Southern Frontier.”⁹⁰ Writer’s cooperatives such as the Tzeltal-Tzotzil workshop Sna Jtz’ibajom (established in 1982), “Strength of the Mayan Woman,” and the Center of Maya and Zoque Writers of Chiapas teach writing in their own languages and produce bilingual publications of poetry, legends, and history.⁹¹ *Jlum jk’inaltik*, or “Tierra Nuestra” (Our Land), the indigenous bulletin of the highlands of Chiapas, emphasizes “the dramatic history of injustice we have lived.”⁹²

These efforts are supported by Mexican and foreign scholars and institutions through the Tzotzil Workshop of the Anthropological Advisory Institute for the

⁸⁵ Frank Cancian, *The Decline of Community in Zinacantan: Economy, Public Life, and Social Stratification, 1960–1987* (Stanford, Calif., 1992).

⁸⁶ Christine Eber, “Making Souls Arrive: Enculturation and Identity in Two Highland Towns,” unpublished paper, 1998, ms. in possession of the author.

⁸⁷ Quoted by Vicente Godínez Valencia, “Chiapas: Iglesia y carisma,” in *Chiapas: Los problemas de fondo*, David Moctezuma Navarro, ed. (Cuernavaca, 1994), 108.

⁸⁸ Collier, *Basta! Land and the Zapatista Rebellion in Chiapas*, 64.

⁸⁹ Petrona de la Cruz Cruz, “El teatro maya de los altos de Chiapas: Su influencia cultural y su futuro,” *Revista de CONSEJO* 8 (March 1993): 15; Eduardo Marcial Corzo, “El teatro regional en Chiapas,” in *Segundo encuentro de intelectuales Chiapas-Centroamérica*, 145–46; and Isabel Juárez Espinosa, *Cuentos y teatro tzeltal: A’yejetik sok ta’jimal cuento* (Mexico City, 1994).

⁹⁰ Rosalva Aída Hernández Castillo, “Cuando el idioma regresó al ejido,” *Ojarasca* 2 (November 1991): 54–56.

⁹¹ Evon Z. Vogt, “The Chiapas Writers’ Cooperative,” *Cultural Survival Quarterly* 9 (1985): 46–48. Sna Jtz’ibajom was organized by Robert Laughlin, who has reported that “the success of this program is an aspect of a native revitalization movement among the Tzotzil and Tzeltal peoples of the Chiapas Highlands.” Vogt, *Fieldwork among the Maya: Reflections on the Harvard Chiapas Project* (Albuquerque, N.Mex., 1994), 344. Also see Antonio de la Torre López, “Chanob Vun ta Batz’i K’op of Sna Jtz’ibajom: An Alternative Education in Our Native Languages,” *Cultural Survival Quarterly* 22 (Spring 1998): 44–45; and Christine E. Eber, “Seeking Justice, Valuing Community: Two Women’s Paths in the Wake of the Zapatista Rebellion,” Working Paper 265, Mankato State University (March 1998), 12.

⁹² Xaw Kojtom Lam, “La voz de nuestro corazón,” *Jlum jk’inaltik* (San Cristóbal de las Casas) (1994): 2; “En la vanguardia: Sna Jtz’ibajom,” *Excelsior*, January 29, 1994; Isaías Hernández Isidro, “Identidad y creación literaria,” *Ojarasca* 37 (October 1994): 57–58; Gordon Brotherston, “Indigenous Literatures and Cultures in Twentieth-Century Latin America,” *The Cambridge History of Latin America*, Vol. 10, Leslie Bethell, ed. (Cambridge, 1995), 296, 301–02.

Maya Region in San Cristóbal, the Regional Center of Multidisciplinary Investigation of the National University, the state university of Chiapas, the Chiapas State Administration for the Strengthening and Promotion of Cultures, the National Alliance of Bilingual Indigenous Professionals, the National Association of Indigenous Language Writers, and the Program of Indian Languages and Literatures of the National Council for Culture and the Arts.⁹³ As Juan Gregorio Regino notes, however, "the process of formation of contemporary indigenous writers does not come out of the universities nor is it a part of an institutional indigenista process, rather it is a product of movements of resistance, self-development, and realization of consciousness."⁹⁴

Critical to indigenous cultural revitalization in Chiapas have been the efforts to overcome the historic divisions of language and community. This process has occurred on its own in the Lacandón, and elsewhere has been actively promoted. In 1984, the first Inter-Ethnic Cultural Encounter was held in San Pedro Chenalhó and obtained the participation of Tzotzil, Tzeltal, Zoque, Mocho, and Cakchikel representatives from thirty-one municipalities. This meeting promoted indigenous music, dancing and other traditional arts and rituals. "At this wonderful fiesta," it was noted, however, "the Indian did not know if he was the guest or the servant. The caution that he arrived with did not let him act freely . . . but now after so many centuries, he is coming to be the protagonist, the director and the soul of his own celebration." This meeting led to the formation of the Committee for the Defense and Strengthening of Indigenous Cultures. The committee dedicated itself to the goal (one among many) that "the young people acquire an understanding of the history of their people and do not lose the traditions that exist in their communities."⁹⁵

This concern for history has been and remains a crucial element of the Maya revival. "The principal point," writes a Yucatec Maya regarding the creation of a new, more just Mexico, "is to study and reconstruct our own history."⁹⁶ We're not going to wait for a foreigner to write our history, Enrique Pérez López writes, "it's important that we Indians be concerned with our own past, that the sources of

⁹³ Natalio Hernández, "La literatura indígena en tiempos de la guerra de Chiapas," *Ojarasca* 45 (August–November 1995): 69–72. The Centro de Investigaciones Humanísticas de Mesoamérica and the state of Chiapas, affiliated with the National Autonomous University of Mexico (UNAM) began in 1987 an annual Concurso de Narrativa Indígena to provide "an open space of expression for Tzotzil, Tzeltal, Chol, Tojolabal and Zoque writers and story tellers that live in Chiapas." *CHIMECH* 3 (January–June 1993): 265–66. On the national scene, *Nuestra Sabiduría*, *Ojarasca*, and *Ce-Acatl* publish indigenous writing.

⁹⁴ Juan Gregorio Regino, "Literatura indígena," *Letras indígenas* 6 (July–August 1994): 1; "La Literatura Indígena Actual, a Debate," *El universal* (March 14, 1994): 3c. "The appearance of a literature in indigenous languages is one of the most important literary phenomena of the end of this century." José Manuel del Val, "Presentación," *Letras indígenas* 1 (September–October 1993): 1.

⁹⁵ Carolina Henríquez A., *El reencuentro de la cultura indígena* (San Cristóbal de las Casas, 1988). This booklet was printed in six languages: Tzeltal, Tzotzil, Tojolabal, Chol, Zoque, and Spanish. The Second Encounter was held in Tecpatán in 1986 and included delegations from nine ethnic groups. The Third Encounter was held in Tenejapa in 1988 and again brought together delegations from nine ethnic groups from every locality in Chiapas.

⁹⁶ Bartolomé Alonso Caamal, "Los mayas en la conciencia nacional," in Warman and Argueta, *Movimientos indígenas contemporáneos en México*, 51; Ricardo Melgar Bao, "Las utopías indígenas en América, lectura de un año nefasto," *Memoria* 62 (January 1994): 29–30.

esteem come from us.”⁹⁷ Indian scholars such as Miguel Hernández, a Tzotzil of Larráinzar, and Jacinto Arias, a Tzotzil of Chenalhó and PhD in anthropology from Princeton University, are leading efforts of “historical redemption,” but many others are showing that one need not have a formal education to narrate or write history.⁹⁸ Many if not most of the new native historians are individuals who have left their ancestral community either voluntarily as migrants to the Lacandón frontier or under force as “expulsados,” those expelled by *caciques* for political and/or religious reasons. In Chiapas, Indian men and women eagerly become historians when given the opportunity.

One of the first and still most ambitious efforts was the collective Zinacantecan history of the Mexican Revolution in Chiapas. From 1976 to 1981, the Tzotzil Workshop led by Andrés Aubry assisted young Zinacantecos in recording the collective memory of the elders (“el Relato de los Ancianos”), who provided accounts typically vague regarding chronology and sequence. One of the elders, however, Don Miguel, “has a historical preoccupation: he wants to know what happened.”⁹⁹ Aubry also provided younger Zinacanteco historians with primary and secondary sources, which they used in conjunction with “the stories of the grandfathers” to reconstruct the history of their community and region from the 1910s to the 1930s. The result was a highly critical Indian commentary on Ladino historiography and the first Zinacanteco history of Zinacantán in the Mexican Revolution. The project, Aubry concluded, “raised the level of the consciousness of the Zinacantecos who participated in the history.”¹⁰⁰

Domingo Gómez Gutiérrez, a Tzeltal from Bachajón, is preparing a book in Tzeltal and Spanish about the Tzeltal Rebellion of 1712 and the “Indian King” Juan López. For many years, this native historian has worked at recovering the various versions of the rebellion that exist in oral tradition in the different highland communities. This eighteenth-century struggle for liberty and justice against the colonial order gave rise to an Indian hero who has never died. “Our grandfathers agree that Juan López lives,” as one informant, Sebastián Guzmán tells it, “that he has not died and is waiting for a time to return.” According to a narration from Chilón, the “Indian King” will come again one day, “come to this land to defend us. Our elderly say that thanks to him Tzeltal Indians at last have respect.”¹⁰¹

Narrating history is not for men only. “This recollection of what has happened,” women historians of two new communities in the Selva Lacandona say, “is for our children, so they will understand that we left home to look for a place where we

⁹⁷ Enrique Pérez López, *Chamula, un pueblo indígena tzotzil* (Tuxtla Gutiérrez, 1990), 185.

⁹⁸ Jacinto Arias, *San Pedro Chenalhó: Algo de su historia, cuentos y costumbres* (Tuxtla Gutiérrez, 1990); Arias, *El mundo numinoso de los mayas* (Tuxtla Gutiérrez, 1991); and Arias, “Movimiento indígenas contemporáneos del estado de Chiapas,” in Warman and Argueta, *Movimientos indígenas contemporáneos en México*, 81–98.

⁹⁹ *K'alal ich'ay mosoal/Cuando dejamos de ser aplastados: La revolución en Chiapas* (Mexico City, 1982), 2: 68. A more recent native account of one significant episode of the Mexican Revolution in Chiapas is *Sventa Pajaro ta Chamula/Los Pajaritos de Chamula*, Gary Gossen, trans. (San Cristóbal de las Casas, 1991).

¹⁰⁰ “¿Qué se pretendió en este libro?” *Cuando dejamos de ser aplastados*, 2: 84. A selection from this book has been translated into English in “The Mexican Revolution in Tzotzil: ‘When We Stopped Being Crushed,’ 1914–1940,” in Womack, *Rebellion in Chiapas*, 97–104.

¹⁰¹ Parts of the narratives compiled by Domingo Gómez are reprinted and discussed by Carlos Montemayor in *Chiapas: La rebelión indígena de México* (Mexico City, 1997), 115–30.

could eat a little better.” Their book comes with many drawings, they noted, so that those who do not read can understand it. And “the words of the history are printed in Tzotzil and in Tzeltal, which are our languages, so that everyone will hear us, even those who don’t speak Spanish.”¹⁰² The residents of one of these communities, Nuevo San Juan Chamula, at first believed that “only *kaxlanes* had written history.” They recognized that “there are people who think very poorly of us, saying that we are only Indian *campesinos*, that we can’t write our words. Although it’s true that we adults don’t know how to read, the young people have gone to school and they help us. We believe that our history doesn’t mean much if it remains guarded in the heart, it’s better if it is written and can go to many places . . . Perhaps if other *campesinos* understand it, they will wake up and come to make their own history.”¹⁰³

The members of “Tierra Tzotzil Community Union” of the El Bosque region told the “history of how we purchased our *finca* [landed estate].” They wanted “[their] thoughts to be taken into account, [their] words to go out.”¹⁰⁴ Those Chamulans who had worked in the German coffee plantations of Soconusco similarly wanted their story known.¹⁰⁵ “The point of writing this history,” wrote a local indigenous historian, “is to show my readers today the drastic changes a group of *campesinos* have lived.”¹⁰⁶ Native historians want to tell the stories of their people in their way, and they want their stories to be widely known, by other Indians and by Mexican society. In this manner, an indigenous historiography is appearing in Chiapas. As Alfredo of Sna Jtz’ibajom says, “these little books are a beginning.”¹⁰⁷

These accounts present a Maya-centric history concerned primarily with how the *kaxlanes* have oppressed the Maya from the time of the Conquest through the present and how the Maya have resisted and persevered. Wars and political conflicts among Ladinos in Chiapas have little meaning and importance except

¹⁰² Garza Caligaris, *Sk’op Antzetik*, 1. This booklet was published in Tzotzil and Tzeltal editions as well as Spanish.

¹⁰³ Anna María Garza, María Fernanda Paz, Juana María Ruiz, and Angelino Calvo, *Voces de la historia: Nuevo San Juan Chamula, nuevo Huixtán, nuevo Matzam* (Cuernavaca, 1994), 20–21. This booklet was first published in a Tzotzil, Tzeltal, and Spanish edition in 1989.

¹⁰⁴ Los socios de la Unión “Tierra Tzotzil,” *Kiipaltik: Lo’il sventa k’ucha’al la jmankutik jpinkakutik*, Salvador Guzmán López and Jan Rus, comps. (San Cristóbal de las Casas, 1990).

¹⁰⁵ Pax Lopes Kalixto, et al., *Abiel ta pinka: Lo’iletik sventa li inyoetik tzotziletik ta pinkaetik sventa kajvel ta Chiapa* (San Cristóbal de las Casas, 1986). A selection from this book has been translated into English in “Migrant Labor on the Coffee Plantations: Debt, Lies, Drink, Hard Work, and the Union, 1920s–1930s,” in Womack, *Rebellion in Chiapas*, 111–18.

¹⁰⁶ Pérez P., *Historia de un pueblo evangélico*, 10.

¹⁰⁷ Alfredo is quoted by Ronald Wright, *Time among the Maya: Travels in Belize, Guatemala, and Mexico* (New York, 1991), 283. José Alejos García, *Wajalix Bat’an: Narrativa tradicional ch’ol de Tumbalá, Chiapas* (Mexico City, 1988); Jacinto Arias, *Historia de la colonia de los Chorro, Chenalhó, Chiapas* (Tuxtla Gutiérrez, n.d.); María Gómez Pérez (with Diana Rus and Salvador Guzmán López), *Ta Jlok’ta chobtik ta k’u’il* (San Cristóbal de las Casas, 1990); Martín Gómez and Enrique Pérez, *K’op a’yejetik sok skuxinel te muk’ul lum tzeltal* (Tuxtla Gutiérrez, 1986); Manuel Hidalgo Pérez, *Tradición oral de San Andrés Larráinzar: Algunas costumbres y relatos tzotziles* (Tuxtla Gutiérrez, 1985); Li’e skuenta sa’k’op vo’ne k’alal imeltzaj ach’ rason: *Revolución mexicana y sus consecuencias entre los tzotziles de Zinacantan* (San Cristóbal de las Casas, 1977); Arturo Lomelí González, *Ayni tuk tradision sok skostumbre ja b’a schonab’il ja tojolab’a’il* (Tuxtla Gutiérrez, 1988); Miguel Meneses López, *K’uk witz, Cerro de los Quetzales: Tradición oral chol del municipio de Tumbalá* (Tuxtla Gutiérrez, 1986); Jesús Morales Bermúdez, *On o t’ian = Antigua palabra: Narrativa indígena chol* (Mexico City, 1984); José Luis Pérez Chacón, *Los choles de Tila y su mundo* (Tuxtla Gutiérrez, 1988); Enrique Pérez López, *Relatos y tradiciones de un pueblo tzeltal* (Tuxtla Gutiérrez, 1986); *Relatos tzeltales y tzotziles/lo’il maxiel: Antología* (Mexico City, 1994). Some of these are “spoken books,” which are translated and transcribed by non-Indians.

when they affect Indian lives. Historians recount the “age of the grandfathers,” when Indian Pueblos owned the land and the men did not have to work on the *fincas*. The epoch of Porfirio Díaz (1876–1911) in Chiapas is also referred to as the “age of the *finsa*,” since by this time rich Ladinos monopolized most of the land. It is remembered as a terrible time of extreme poverty, physical abuse, and virtual slavery under a system of debt servitude. Liberation then seemed possible because of the actions of local and national revolutionary heroes. The Mexican Revolution (1910–1920) in Chiapas, however, involved struggles among Ladino factions that only vandalized native communities; it is remembered as “the time of hunger.” Native accounts generally distinguish the age before and after the “Revolution of the Indians” (agrarian reform and the formation of the indigenous labor union) of the 1930s. For a time, the *ejidos* had enough good land, the labor union worked as it should, and life was better. But the revolutionaries left the scene, officials again cheated Indians, and good land passed to the Ladinos. The present is another time of hardship and oppression for the Maya, who confront it through the organization of unions and cooperatives, struggle against corrupt officials and landowners, and migration to the jungle. Their history therefore justifies Maya resentment, reinforces their demands for land, education, and autonomy, and, finally, motivates and inspires their current struggles for justice.¹⁰⁸

Maya-centric history is fusing with revisionist national history to create a new historical syncretism. This is consistent with the Indian desire to be a *people* with some measure of autonomy as well as be *citizens* with equal rights.¹⁰⁹ Revolutionary Mexican history provides examples and role models: the resistance of Cuauhtémoc against the Spanish conquest, the insurgency of Miguel Hidalgo and José María Morelos in 1810 for national independence, the wars of Benito Juárez for ideological definition and national self-determination, and particularly the Revolution of 1910 and the struggle of Emiliano Zapata for social justice.¹¹⁰ The struggle of Zapata is held up as an example of *campesinos* and Indians effectively confronting the ruling elite and the state, making a difference, and changing history for the better. Throughout Mexico in the 1970s and 1980s, peasant and Indian organizations have appropriated that revolutionary name, image, and history.¹¹¹ “In a country of symbols,” comments Paco Ignacio Taibo, “reality is eminently symbolic.”¹¹² As even President Carlos Salinas stated in his fifth state of the union address in 1993, unknowingly prescient, “in our nation there will always be battles

¹⁰⁸ The new indigenous literature includes traditional (sacred) narratives. There is a clear determination to maintain and increase respect for “our cosmology.” See J. L. Pérez Chacón, *Antigua palabra maya: Literatura tzotzil* (Tuxtla Gutiérrez, 1988).

¹⁰⁹ Adolfo Gilly, *Chiapas: La razón ardiente: Ensayo sobre la rebelión del mundo encantado* (Mexico City, 1997), 97.

¹¹⁰ The four Declarations of the Selva Lacandona by the Zapatista Army of National Liberation (EZLN) begin with references to or statements by these national heroes.

¹¹¹ The most important of these is the Plan de Ayala National Coordinating Body (Coordinadora Nacional Plan de Ayala, CNPA) formed in Milpa Alta, D.F., in 1979. The name is taken from Zapata’s 1911 revolutionary declaration and program. In Chiapas, one of the most important agrarian organizations is the Organización Campesina Emiliano Zapata, formed in 1982, which is affiliated with the CNPA. The way in which “Zapata occupies a complex space in Mexico’s soul” is discussed by Anthony DePalma, “In the War Cry of the Indians, Zapata Rides Again,” *New York Times*, January 27, 1994.

¹¹² Taibo quoted by Méndez Asensio and Cano Gimeno, *La guerra contra tiempo*, 102; Luis Hernández Navarro, “El fantasma de Zapata,” *La jornada*, July 16, 1994.

for social justice, so long as the memory and example of Emiliano Zapata remains in Mexicans' hearts."¹¹³

The Emiliano Zapata National Independent Peasant Alliance (ANCIEZ), born in the south of Puebla in 1991 to push for agrarian reform, credit, and democracy, immediately took root in the Selva Lacandona, spread to Yajalón in the north and Larráinzar and Huixtán in the highlands, and centered its operations in Altamirano. It was led by Jesús Santis Chus, a Tzeltal from the *ejido* Morelia and included, like many similar organizations in Chiapas, leftist intellectuals and professionals from other regions of Mexico interested in grass-roots political organizing. In the spring of 1992, ANCIEZ and other agrarian groups organized in Ocosingo the commemoration of the assassination of Zapata and a protest against both the forthcoming North American Free Trade Agreement (NAFTA) and the reform (essentially the revocation) of Article 27, the constitutional provision for agrarian reform. More than 3,000 Indians appeared on April 10 led by Francisco Gómez, ANCIEZ's representative in Ocosingo. The march concluded with a public reading of a letter by Gómez to the president. "A few months ago," he protested, "our most precious historical conquest was extinguished: the right to land."¹¹⁴

ANCIEZ was one of seventeen organizations in Chiapas that came together in 1992 to protest the Columbian quincentennial as part of the Common Front of Social Organizations—500 Years of Struggle and Resistance of the Chiapas People. The quincentennial spurred Indian protest throughout the Americas from Minnesota to Managua. An Indigenous Congress in Guatemala City one year earlier proposed a "counter-Quincentennial" and Indian activists carried it out in the Andes and on the Amazon, in Santo Domingo and Mexico City, with peaceful protests and some violent acts on October 12.¹¹⁵ The Mexican Council of 500 Years of Indian, Black and Popular Resistance coordinated marches and protests against the "Western farce" of the quincentennial.¹¹⁶ In the city of Morelia, Michoacán, Indian protesters pulled down the statue of a Spanish viceroy. Thousands of Indians from all parts of Mexico gathered in the great ancient city of Teotihuacán in the valley of Mexico and later attacked the Columbus monument in downtown Mexico City. The council's slogan, "Never Again a Mexico Without Us," revealed the nationalist solidarity of Mexico's indigenous movements.¹¹⁷

In Chiapas, the Common Front of Social Organizations planned a protest march of thousands through San Cristóbal to mark five centuries of indigenous resis-

¹¹³ Salinas quoted by Elsie L. Montiel, "Chronicle of a Conflict Foretold," *Voices of Mexico* 27 (April–June 1994): 84.

¹¹⁴ Tello Díaz, *La rebelión de las Cañadas*, 132–33, 148–49. The amendment of Article 27 of the 1917 constitution removed the right of citizens to petition for land redistribution and permitted the partial privatization of *ejido* land to encourage investment in agriculture. The dismantling of Article 27 and the prospect of NAFTA was viewed by most mestizo and Indian *campesinos* in Mexico and Chiapas as serious threats to farming as a way of life.

¹¹⁵ Enrique Plasencia de la Parra, "Introducción," *La invención del quinto centenario: Antología* (Mexico City, 1996), 9–44.

¹¹⁶ Matilde Pérez U., "Se suman grupos Mayas a la marcha de la dignidad indígena," *La jornada*, October 4, 1992.

¹¹⁷ Raúl Llanos Samaniego, "No al quinto centenario, demanda desde Chapultepec hasta el Zócalo," *La jornada*, October 13, 1992; "Manifiesto del Consejo Mexicano 500 Años de Resistencia India, Negra y Popular al Pueblo Mexicano," México, Tenochtitlán, October 12, 1992, handbill.

tance.¹¹⁸ Although there were events in numerous towns in Chiapas on October 12, more than 10,000 natives from nearly every corner of the state gathered in the city founded by Mazariegos.¹¹⁹ Bishop Ruiz celebrated Mass on the previous evening and in his homily referred to 500 years of suffering and resistance that continued still.¹²⁰ About half the marchers were ANCIEZ militants armed with bows and arrows and signs denouncing NAFTA, Salinas's neoliberal policies, and "500 YEARS OF ROBBERY, MURDER, AND DESTRUCTION OF THE INDIGENOUS PEOPLE." The ANCIEZ contingent was particularly notable for the large number of women in its ranks. The march began at the 1974 monument to Bishop Las Casas and proceeded to the central square, the 31st of March Plaza, named for the date of Mazariegos's founding of the city. Shouts of "Columbus Against the Wall" were heard from the crowd. Some women carried the popular flag of Indian Mexico, a green, white, and red banner with the Virgin of Guadalupe in the center. Pedro Gómez López, president of the Zoque Supreme Council, noted that "this is the best date to appreciate the real events that engaged the lives of the Indians of this continent in the bloody massacre that made them victims."¹²¹

The marchers continued through town to Santo Domingo Church, where the monument to Mazariegos was located. The monument stood in front of what is today the Chiapas Highlands Museum in the ancient Dominican convent but had been for years the city jail. ANCIEZ militants immediately surrounded the monument and climbed upon it to chants of "abajo, abajo" (down with it!). While city policemen watched, some men pulled at the statue with ropes and others attacked it with a sledgehammer. In about ten minutes, the green figure was on the ground. It was doused with gasoline and set on fire, but the damage was light. Marchers then hoisted the statue on their shoulders and carried it to city hall. There they set upon it with hammers, smashed it to pieces and sold them to onlookers. The march ended with speeches—in Tzotzil, Tzeltal, and Tojolabal—declaring that Indian unity was the only way to end five centuries of injustice.¹²²

What did this mean? *Coletos* blamed Bishop Samuel Ruiz, "the antichrist of San Cristóbal," and his "communist priests" for fomenting a rebellion. Socorro Zebadúa Celoria, head of San Cristóbal's national chamber of commerce chapter, complained that the clergy had provoked Indians and led them to believe they would not be held responsible for their actions. City authorities characterized it as vandalism and the work of "professional agitators." Furthermore, noted Jorge Marío Lescieur Talavera, the municipal president, the march itself included two

¹¹⁸ Matilde Pérez U., "Algo podría suceder por la situación en Chiapas, el gobernador," *La jornada*, October 11, 1992.

¹¹⁹ Quincentennial protests took place in Venustiano Carranza, Palenque, Ocosingo, Simojovel, Salto de Agua, Las Margaritas, Motozintla, Sabanilla, Tumbalá, and Marqués de Comillas.

¹²⁰ Matilde Pérez U., "No perder identidad, pide obispo de San Cristóbal a los indígenas," *La jornada*, October 12, 1992.

¹²¹ "Ataques a monumentos en las marchas contra el quinto centenario," *Unomásuno* (Mexico City), October 13, 1992. There are only two, very brief accounts of the events of October 12 in print: Tello Díaz, *La rebelión de las Cañadas*, 151–52; and Ross, *Rebellion from the Roots*, 80–82.

¹²² Interview with a Chamula living in San Cristóbal who participated in the march. San Cristóbal, July 1994. Days earlier, cattlemen from Ocosingo warned of "bloody deeds" on October 12 in an open letter and demanded the intervention of the army to prevent violence and land takeovers. Matilde Pérez U., "Exigen seguridad y garantías para los cerca de 800 integrantes de la marcha de la dignidad indígena," *La jornada*, October 6, 1992.



Base of the former Mazariegos monument at Santo Domingo Church, San Cristóbal de las Casas. Photograph by Sharon Lee House, July 1994.

priests and had been incited by “the political clergy of the diocese of San Cristóbal.” The municipal president added that the monument was an important “tourist attraction,” since “in all of Latin America there existed only two statues of conquistadors, the one of Diego de Mazariegos, in San Cristóbal, and that of Francisco Pizarro, in the capital of Peru.”¹²³ Lescieur Talavera with other “authen-

¹²³ César Espinosa, “Demanda contra quines destruyeron la estatua de Diego de Mazariegos,” *El día*

tic residents" immediately formed the United Front for Citizens' Defense, which initiated a propaganda campaign to have Bishop Ruiz removed from office and expelled from San Cristóbal and Chiapas.¹²⁴

The destruction of the monument was not a spontaneous action of the crowd. ANCIEZ militants, according to other participants of the march, proceeded directly to the monument and destroyed it without hesitation or discussion. It was later learned that some of the leaders of the underground and as yet unknown Zapatista Army of National Liberation (EZLN) were present in San Cristóbal on that day, including the soon-to-be famous Subcomandante Marcos, and were prepared to strike back in case of police repression. Many ANCIEZ leaders and followers, furthermore, were at this time, or soon would be, Zapatistas. Subcomandante Marcos subsequently noted that the Zapatista civilian population voted in the fall of 1992: "And the result, by several tens of thousands, was that the war would have to start, in October of 1992, with the quincentenary."¹²⁵

The participants of the march understood the significance of their overwhelming presence and dramatic actions on that day. Interviews conducted after the event reveal that Indian marchers were aware that they were present at a historic occasion and were proud to claim attendance. Those who later agreed to be interviewed demonstrated a solid understanding of the meaning of "the discovery" of October 12, 1492, the conquest by Mazariegos in 1528, and the humanitarianism of Bartolomé de las Casas. Marchers were offended that Mazariegos was a hero to the Ladinos of San Cristóbal.¹²⁶ One image repeatedly found in the testimony of informants is telling: Mazariegos was an object of Indian wrath because he was a *conquistador de indígenas* who branded Indian slaves on their foreheads with a burning iron.¹²⁷

People who experienced persecution, expulsions, jailing, violence, and worse offenses as a reality of their lives had little difficulty understanding the conquest that their ancestors in the late 1520s suffered. They had little difficulty understand-

(Mexico City), October 15, 1992; Matilde Pérez U., "Pólemica por la destrucción de la estatua de Diego de Mazariegos," *La jornada*, October 14, 1992. Even before the event, the editor of San Cristóbal's *La voz del Sureste* wrote that the bishop "incites Indians to violence." Quoted by Pérez U., "Algo podría suceder por la situación en Chiapas."

¹²⁴ Two years later, the Frente Unico was one of more than a hundred organizations representing supposedly 120,000 citizens that came together in a coalition in "defense of law and the constitution." This citizens' group used the phrase "defense of law" as a euphemism for police and army action against Zapatista rebels and other Indian-rights organizations and for efforts to force Bishop Ruiz out of Chiapas. A handbill distributed in San Cristóbal stated that "we know that in Chiapas today no one is going to defend our rights, and the time has come for us to defend ourselves." From "Coalición de Organización Ciudadanas del Estado de Chiapas por la Defensa de la Ley y la Constitución," July 1994, handbill.

¹²⁵ Subcomandante Marcos, "Carta a Adolfo Gilly," October 22, 1994, quoted in Adolfo Gilly, "Chiapas and the Rebellion of the Enchanted World," in Daniel Nugent, ed., *Rural Revolt in Mexico: U.S. Intervention and the Domain of Subaltern Politics* (Durham, N.C., 1998), 301.

¹²⁶ Interviews with native informants in San Cristóbal and Ocosingo in July 1994 and December 1995 were conducted with the stipulation that no names be mentioned.

¹²⁷ In the Chiapas Highlands Museum in the Dominican Convent in San Cristóbal, there is a reproduction of a section of a Diego Rivera mural showing a Spanish *encomendero* branding an Indian. Bishop Samuel Ruiz talked about the branding of Indian slaves as one consequence of the Conquest in the documentary film *Columbus and the Age of Discovery*, Episode 5, "The Sword and the Cross," PBS, 1992.

ing “the long history of the struggle which the indigenous peoples have been waging for five hundred years.”¹²⁸ They had great difficulty, however, understanding why anyone would wish to celebrate 1492 or memorialize the conqueror of Chiapas. “The native people of Chiapas refuse to celebrate the 500 years encounter of two worlds,” declared Jacinto Arias Pérez, “rather they consider it a celebration of aggression against the Indian peoples of Mexico.”¹²⁹ It was in this context and precisely at this time that the Indian communities in the Selva Lacandona began to discuss and debate the necessity of armed rebellion. Local assemblies analyzed the risks of rebellion, and a majority of communities in the region in late 1992 voted for war. In January 1993, the Clandestine Indigenous Revolutionary Committee of the Zapatista Army was organized and preparations for the uprising were begun.¹³⁰

ONE YEAR AND TWO MONTHS AFTER THE FALL of the statue to the conquistador, Indian soldiers seized San Cristóbal for the first time in history. The Zapatista rebellion that began on New Year's Day in 1994 was a brief war and an extended history lesson. The rebel's spokesman, Subcomandante Marcos, writes Alma Guillermoprieto, “staged a very real, threatening war on the Mexican state based on almost no firepower and a brilliant use of Mexicans' most resonant images: the Revolution, the peasants' unending struggle for dignity and recognition, the betrayed Emiliano Zapata.”¹³¹ The first paragraph of the Zapatista manifesto, “The Declaration of the Lacandón Jungle,” recounted the history of popular Mexican struggles against Spain, the United States, France, and the dictator Porfirio Díaz.¹³² The name of the rebels' official organ, *El despertador mexicano*—the Mexican Alarm—was a direct reference to the insurgent newspaper of the 1810 revolution for independence.¹³³ “The Zapatista rebels are thus Indians who have believed in Mexico's public

¹²⁸ “Interview: Emiliano Zapata Campesino Organization,” in *First World, Ha Ha Ha!* 137.

¹²⁹ Arias Pérez quoted by Rita Balboa and Gonzalo Egremy, “Chiapas niegan su identidad para no ser tratados como ‘indios,’” *El universal*, October 12, 1992.

¹³⁰ Montemayor, *Chiapas: La rebelión indígena de México*, 138–40; Obregón R., “La rebelión zapatista en Chiapas,” 186. The EZLN was formally established in 1983.

¹³¹ Alma Guillermoprieto, “The Unmasking,” *New Yorker* (March 13, 1995): 42. Guillermoprieto has called the rebellion “a shadow war.” See “The Shadow War,” *New York Review of Books* (March 2, 1995): 34–43; and Alberto Cue's interview of Jorge Aguilar Mora, “Guerra zapatista en México: Modernidad y posmodernidad,” *La jornada semanal* (August 7, 1994): 22–30. Guillermo Gómez Peña has written that “what made the Zapatista insurrection different from any other recent Latin American guerrilla movement was its selfconscious and sophisticated use of the media.” “The Subcomandante of Performance,” *First World, Ha Ha Ha!* 90.

¹³² The Declaration of the Lacandón Jungle begins, “We are the product of 500 years of struggle.” Oscar Camacho Guzmán, “‘Declaración de guerra’ del Ejército Zapatista en Chiapas,” *La jornada* (January 2, 1994): 8.

¹³³ The newspaper of Father Miguel Hidalgo's insurgency published in Guadalajara in 1810 was entitled *El despertador americano*. Throughout Mexico and the weeks and months after the uprising, peasant groups took action against local political bosses and government offices. “The word they used again and again was ‘awakened.’ That was what the Zapatistas, they said, had done to them.” Tim Golden, “‘Awakened’ Peasant Farmers Overrunning Mexican Towns,” *New York Times*, February 9, 1994. In August 1994, the Zapatistas organized a National Democratic Convention in a jungle site named “Aguascalientes” in reference to the revolutionary convention of Aguascalientes in 1914. The word “convention,” writes Andrés Aubry, brings to mind Zapata, Villa, Carranza, and Obregón. “Convención: Las experiencias de la historia,” *La jornada*, July 3, 1994.

Revolutionary rhetoric," writes Gary Gossen, "but who now feel utterly betrayed by the nation's revised priorities."¹³⁴

No history was more important to the Zapatistas than the history of the first and original Zapatistas (and no name more subversive to the government and establishment media).¹³⁵ On April 10, 1994, the seventy-fifth anniversary of Zapata's assassination, the Zapatista forces staged an impressive ceremony in rebel territory before local residents and the international media. Three or four hundred Zapatista soldiers paraded before the movement's supreme council while musicians played the national anthem. Three speakers commemorated the life and death of the "apostle of agrarianism" and affirmed the Zapatista principle that in 1994 as in 1919 the land belongs to him who works it. "Our heart is happy," Marcos proclaimed, "because Emiliano Zapata has returned."¹³⁶ An elderly man with a distinctive Zapata-like mustache, a representative of the Zapatista veterans of 1910, reviewed the troops. Later that evening, revolutionary ballads were played while soldiers danced with young women in their best dresses. "How long has it been," wondered one observer, "since we've seen such a moving April 10 in Mexico?"¹³⁷

The new historical syncretism combining national and indigenous history was highlighted in the EZLN's creation of Votán-Zapata. Votán, "Guardian and Heart of the People," the Tzeltal name of the principal deity of ancient Chiapas as well as, possibly, a historical holy man, was united symbolically with Emiliano Zapata, Mexican history's most respected revolutionary. Both were leaders (*caudillos*) and liberators.¹³⁸ "There was a man who, his word traveling from far away, came to our mountains and spoke with the tongue of true men and women," Marcos proclaimed on behalf of the Clandestine Committee in 1994. One year later, he further clarified this symbolic partnership: "In us, in our arms, in our covered face, in our true word, Zapata united the wisdom and the struggle of our most ancient ancestors. Joined with Votán, the Guardian and Heart of the People, Zapata rose up again to struggle for democracy, liberty and justice for all Mexicans. Although he has Indian blood, Votán-Zapata does not struggle only for the indigenous, he also struggles for those who are not indigenous but who live in the same misery."¹³⁹

The study of history played a role in the Zapatista preparations for war. The

¹³⁴ Gary Gossen, "Comments on the Zapatista Movement," *Cultural Survival Quarterly* 18 (Spring 1994): 20.

¹³⁵ Government officials referred to the rebels as "delinquents" and "law breakers," and television and radio reporters were ordered by their directors not to use the name "Zapatista." See "El termino Ejército Zapatista, prohibido en radio y televisión," *La jornada*, January 12, 1994.

¹³⁶ "Communique from the Clandestine Indigenous Revolutionary Committee of the Zapatista National Liberation Army, Mexico, April 19, 1994."

¹³⁷ Hermann Bellinghausen, "Los rostros verdaderos," *Ojarasca* 33–34 (June–July 1994): 31; "Bienvenidos a la cuna de insurgentes," *Macrópolis* 118 (June 20, 1994): 21–22.

¹³⁸ Edgar Robledo Santiago, "Votán," in *Lecturas Chiapanecas* (Mexico City, 1980), 58–60; and Eva Hunt, *The Transformation of the Hummingbird: Cultural Roots of a Zinacantecan Mythical Poem* (Ithaca, N.Y., 1977), 133.

¹³⁹ "Votán-Zapata, 11 de abril," *EZLN: Documentos y comunicados* (Mexico City, 1994), 210–13; an English translation is found in *Shadows of Tender Fury: The Letters and Communiqués of Subcomandante Marcos and the Zapatista Army of National Liberation*, Frank Bardacke, Leslie Lopez, and the Watsonville, California, Human Rights Committee, trans. (New York, 1995), 195–98; and "Votán-Zapata se levantó de nuevo, 16 de abril de 1995," *EZLN: Documentos y comunicados*, vol. 2 (Mexico City, 1995), 306–09. Marcos discussed the fusion of Zapata and Votán in the October 24, 1994, interview in Gilly, Marcos, and Ginzburg, *Discusión sobre la historia*, 134.

development of political consciousness of communities connected to Zapatismo, one investigator argues, “implied the recuperation of the past.”¹⁴⁰ Marcos told how he “went to teach what the people wanted: literacy and Mexican history.”¹⁴¹ Indian soldiers interviewed after the uprising told how they received instruction in using firearms, reading and writing, and Mexican history.¹⁴² Lieutenant Amalia recalled how, at age seventeen, after learning a little Spanish, “we began to study the history of Mexico . . . And after that we were taught combat tactics.”¹⁴³ Comandante Ramona similarly noted the importance of Mexican history (and that of other liberation struggles) in the formation of a rebel.¹⁴⁴ In the months following the uprising, dozens of journalists penetrated the Lacandón and surrounding valleys searching for Zapatistas to interview. Those who talked to them, like Mayor Moisés, knew something about Mexico’s revolutionary history. We Mexicans have rebelled many times, he noted, “it is a long history, coming from the struggles against the Spanish in 1810, and from the struggles against the dictatorship of Porfirio Díaz, in the year 1910. From Zapata and from Villa. The time has come again to form an army, an army of the people.”¹⁴⁵ Moisés noted that the EZLN “educated me,” and a reporter remarked that “he narrated entire episodes of national history.”¹⁴⁶

A small episode during the Zapatista occupation of San Cristóbal illustrates the significance of history to the rebels. On January 1, 1994, rebel forces seized city hall and began destroying land titles and other administrative papers. After a plea by the archivist and a brief consultation with the Clandestine Committee, the Indian army took special care to protect the municipal historical archive. Every office in the building save the archive was destroyed and its contents burned or dumped into the plaza. One *comandante* in patchy Spanish stated: “We too respect the history of the people, we are not fighting against them. This archive shows us the historical struggle and advancement of the people and we will not destroy it.”¹⁴⁷

The rebellion itself has further encouraged the study, teaching, and writing of history. The Zapatista delegation in the 1995 negotiations with the national government at San Andres Larráinzar demanded the recuperation of native historical sources and the elaboration of native histories by native historians.¹⁴⁸ In the highlands, an “awakening” of the desire to study the history of indigenous people has been reported.¹⁴⁹ The significance of history and the construction of a usable past as part of an ethnic renaissance is certainly not restricted to Chiapas.

¹⁴⁰ Guiomar Rovira, *Mujeres de Maíz* (Mexico City, 1997), 49.

¹⁴¹ Marcos quoted in Harvey, *Chiapas Rebellion*, 165.

¹⁴² “The preparation of the combatants was very strict.” It included technical understanding of weapons, the development of revolutionary ideas from the early utopian socialists to the disciples of Karl Marx, and the history of Mexico. Tello Díaz, *La rebelión de las Cañadas*, 176.

¹⁴³ Bellinghausen, “Los rostros verdaderos,” 28. Captain Laura told Guiomar Rovira, “in the mountains we learned many things, history, for example.” Rovira, *Mujeres de Maíz*, 75.

¹⁴⁴ Interview by Matilde Pérez U. and Laura Castellanos in *Doble jornada*, March 7, 1994.

¹⁴⁵ Guido Camú and Dauno Tótoro, “Mayor Moisés,” *Macrópolis* 102 (February 28, 1994): 30.

¹⁴⁶ Rebeca Hernández, “Lluvia, zozobra, soledad y hambre del guerrillero,” in Luis Humberto González, ed., *Los torrentes de la sierra: Rebelión zapatista en Chiapas* (Mexico City, 1994), 70.

¹⁴⁷ Quoted in Ross, *Rebellion from the Roots*, 16. “Respetaron rebeldes el archivo histórico de San Cristóbal,” *La jornada*, January 12, 1994.

¹⁴⁸ Rosa Rojas, *Chiapas ¿Y las mujeres qué?* vol. 2 (Mexico City, 1995), 262–64.

¹⁴⁹ Eber, “Seeking Justice, Valuing Community,” 38.

Native peoples throughout the Americas have also awakened to the significance of their past.

"Today, at the hour of our awakening," Justino Quispe proclaimed at the first Indian Congress of South America in October 1974, "we must be our own historians."¹⁵⁰ From the Quichua of the Ecuadorian Andes and the Miskitu of the Atlantic coast of Nicaragua to the Blackfoot of Montana, native peoples throughout the Americas are newly employing history to strengthen and revitalize ethnicity, identity, and culture.¹⁵¹ This is particularly true for the Maya and not simply the Maya of Chiapas. Yucatecan Maya, angry about official history that "has hidden the valiant nature of our people," demand a new curriculum, a Maya curriculum in language, culture, and history.¹⁵² The most extensive study and writing of native history by Mayan people is now developing in Guatemala. There, Kay B. Warren has found that "Mayas want to know what others have written about them, unmask foreign researchers' politics and identities, discuss among themselves the psychological scars of racism, and generate their own cultural knowledge."¹⁵³ Herminio Pérez, a Maya radio broadcaster in Guatemala, explains, "for us the past is a tool to analyze the present in order to plan the future."¹⁵⁴ In Guatemala, Yucatán, and Chiapas, Mayas are challenging the historical narratives that have assumed their conquest and justified Ladino domination, and are rewriting history.¹⁵⁵

The rise and fall of the monument to Captain Diego de Mazariegos frames the first stage of the Maya revival in Chiapas. When the municipal government erected the statue in 1978, only four years had passed since the meeting of the first Indigenous Congress in San Cristóbal de las Casas. During the 1970s and 1980s, the consequences of the congress reverberated throughout Chiapas like aftershocks following an earthquake. One of those was the origin of an indigenous historiography. The native people of Chiapas had long been considered a "people without history." The colonial and national state, writes Julio Atenco, "has attempted to

¹⁵⁰ Quoted by Michel de Certeau, *Heterologies: Discourses on the Other*, Brian Massumi, trans. (Minneapolis, 1986), 227.

¹⁵¹ See Ernesto Salazar, *Indian Federation of Ecuador*, Cultural Survival, International Working Group on Indigenous Affairs, Paper No. 28, 1987; Susan Hawley, "Protestantism and Indigenous Mobilisation: The Moravian Church among the Miskitu Indians of Nicaragua," *Journal of Latin American Studies* 29 (1997): 111–29; Lea Whitford, "Teaching Tribal Histories from a Native Perspective," *Cultural Survival Quarterly* 22 (Spring 1998): 35–37; R. David Edmunds, "Native Americans, New Voices: American Indian History, 1895–1995," *AHR* 100 (June 1995): 717–40; Patricia Galeana, "El neoindigenismo en México," and Miguel León Portilla, "La antigua y la nueva palabra de los pueblos indígenas," in *Cuadernos americanos* 59 (September–October 1996): 164–83, and 196–201.

¹⁵² Allan Burns, "Maya Education and Pan Maya Ideology in the Yucatán," *Cultural Survival Quarterly* 22 (Spring 1998): 50–52.

¹⁵³ Kay B. Warren, "Transforming Memories and Histories: The Meaning of Ethnic Resurgence for Mayan Indians," in Alfred Stepan, ed., *Americas: New Interpretive Essays* (New York, 1992), 189–219; also see Richard Wilson, *Maya Resurgence in Guatemala: Q'eqchi' Experiences* (Norman, Okla., 1995); and María del Carmen León, Mario Humberto Ruz, and José Alejos García, *Del katún al siglo: Tiempos de colonialismo y resistencia entre los mayas* (Mexico City, 1992).

¹⁵⁴ Phillip Wearne, *Return of the Indian: Conquest and Revival in the Americas* (Philadelphia, 1996), 176; Edward F. Fischer and R. McKenna Brown, "Introduction: Maya Cultural Activism in Guatemala," and Kay B. Warren, "Reading History as Resistance: Maya Public Intellectuals in Guatemala," in Fischer and Brown, eds., *Maya Cultural Activism in Guatemala* (Austin, Tex., 1996), 15–16, 89–106; and Warren, *Indigenous Movements and Their Critics: Pan-Maya Activism in Guatemala* (Princeton, N.J., 1998).

¹⁵⁵ See the section "New Indian Writing in Mesoamerica," in Carmack, Casco, and Gossen, *Legacy of Mesoamerica*, 467–71.

erase historical memory, making Indian initiatives to reconstruct it impossible.”¹⁵⁶ In Chiapas, Indian initiatives to write history were undertaken for the first time since the ancient Maya wrote in stone. Indians have passed from being objects of someone else’s history to become the subjects of their own history. The new histories of the Maya portray Indians as protagonists of the history of Chiapas and Mexico. As protagonists, they see themselves more clearly, with greater unity and with greater pride. The Chamula historian Enrique Pérez López writes, “we have come to know ourselves better.”¹⁵⁷ History helped produce, noted Antonio Hernández Cruz, “more awareness of our identity.”¹⁵⁸ That identity is both indigenous and national, Mayan and Mexican, grounded in local indigenous history and energized by national revolutionary history.¹⁵⁹ As Captain Cristóbal proudly announced, “We are natives, we are *campesinos*, we are Mexicans.”¹⁶⁰ This new historical identity is critical to Indians’ ability to place recognition of the rights of indigenous peoples on the agenda of a democratizing Mexico.¹⁶¹ In this context, the destruction of the monument to the conquistador in 1992 was itself a revisionist historical statement: the history of Chiapas is a work in progress.

¹⁵⁶ Julio Atenco, “Un estado de cuenta,” *Ojarasca* 45 (August–November 1995): 13.

¹⁵⁷ Pérez López quoted by de Vos, *Vivir en frontera*, 31. “A new identity has been born. The process took off with the Indigenous Congress in October 1974 and culminated in January 1994 with the armed uprising.” Maza, “Juntas, la acción política y la acción pastoral,” 25. Also see Enrique Rajchenberg and Catherine Héau-Lambert, “History and Symbolism in the Zapatista Movement,” in John Holloway and Eloina Peláez, eds., *Zapatista! Reinventing Revolution in Mexico* (London, 1998), 19–38.

¹⁵⁸ “Interview with Antonio Hernández Cruz of CIOAC,” *Abya Yala News* 8 (Summer 1994). Luis Hernández Navarro, “Reconstrucción de las identidades indias,” *La jornada*, July 19, 1995.

¹⁵⁹ This national identification is not found in all Indian revitalization movements. Héctor Díaz Polanco identifies one current of the new indigenism as “ethnicism,” which attributes a Western character to the nation and to national cultures and thus repudiates any national solutions. See Díaz Polanco, *Indigenous Peoples in Latin America*, 73–74.

¹⁶⁰ Ricardo del Muro, “Encuentro con los Zapatistas,” *Macrópolis* 97 (January 24, 1994): 32. Also see the testimony of Maríán Peres Tzu, trans. by Jan Rus, “The First Two Months of the Zapatistas: A Tzotzil Chronicle,” in Kevin Gosner and Arij Ouweeneel, eds., *Indigenous Revolts in Chiapas and the Andean Highlands* (Amsterdam, 1996), 120–30; and Arij Ouweeneel, “Away from Prying Eyes: The Zapatista Revolt of 1994,” in Gosner and Ouweeneel, 94–101.

¹⁶¹ Xochitl Leyva Solano, “The New Zapatista Movement: Political Levels, Actors and Political Discourse in Contemporary Mexico,” in Valentina Napolitano and Xochitl Leyva Solano, eds., *Encuentros Antropológicos: Power, Identity and Mobility in Mexican Society* (London, 1998), 35–53.

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AHR Forum
Crossing Slavery's Boundaries

*Finding ways to understand the history of slavery is the central issue in this Forum. **David Brion Davis** presents the main essay on this critical but elusive subject. He draws on his experiences as a scholar and teacher to argue for a fundamental shift in our pedagogical and research approaches to slavery. Slavery must be studied, he insists, from a “big picture” to counter the parochialism that has dominated the subject, particularly in the United States. By a “big picture,” he means that historians must cross the boundaries of colonies, nations, and regions to recover the history of slavery because the institution itself traversed the globe. Davis offers specific examples of how slavery can be studied and taught in a broad perspective. His examples make the development of the institution its central focus rather than its particular expression in a certain place or time. His expansive approach to the spatial and temporal history of slavery wins the endorsement and critical reflection of three commentators, **Peter Kolchin, Rebecca J. Scott, and Stanley L. Engerman**. They engage Davis’s call for the transnational study of slavery by raising questions about the very notion of a broad perspective on slavery, the inherent problems of definition that bedevil any attempt to study slavery in transnational terms, the place of individual slaves and their experiences in such a “big picture history,” and the relationship of slavery however broadly defined to various forms of unfree labor. Davis’s passion to understand slavery infuses the entire Forum with a sense of mission and purpose. It compels us to reconsider not just how we teach and write about slavery, but how we think about past boundaries of all kinds.*

AHR Forum
Looking at Slavery from Broader Perspectives

DAVID BRION DAVIS

IN 1944, THE YEAR WHEN ERIC WILLIAMS'S *Capitalism and Slavery* dramatized some of the broad and central meanings of the Atlantic Slave System, Gunnar Myrdal noted the tendency of Americans "to localize and demarcate the Negro problem," which was nevertheless "*an integral part of . . . the whole complex of problems in the larger American civilization.*"¹ Yet despite such influential voices as Myrdal and Williams, American Negro slavery, often seen as the source of what white Americans termed "the Negro problem," continued to be studied and taught as a chapter in the history of the U.S. South. Even in 1998, after more than three decades of voluminous scholarly research and publication, the average American, upon hearing the words "African-American slavery," will almost certainly think of the South and the Civil War. Many of today's college students would also think of a subject now departmentalized in African-American Studies.

I can think of two ways of looking at slavery from broader perspectives that help us move beyond the localizing and demarcating tendencies that Myrdal described. Of course, specialized studies—such as the examinations of slavery in Spanish Florida, French Louisiana, and Dutch New Amsterdam that Ira Berlin drew on for his monumental book *Many Thousands Gone*—are indispensable for any broader syntheses.² That said, the first method I have in mind is the comparative approach in which two or more examples of human bondage are compared, analyzed, and contrasted, as in the pioneering works by Frank Tannenbaum and Stanley Elkins.³ The second method is to take a global or multinational view of the origins, development, and abolition of racial slavery in the New World, which can be thought of as a way of gaining broader insight into world history and the human costs of "modernization."⁴

The essays in this *Forum* were originally read as papers, in somewhat different form, at a session of the annual meeting of the Organization of American Historians, April 3, 1998, in Indianapolis.

¹ Gunnar Myrdal, *et al.*, *An American Dilemma: The Negro Problem and Modern Democracy* (New York, 1944), liii, italics in original.

² Ira Berlin, *Many Thousands Gone: The First Two Centuries of Slavery in North America* (Cambridge, Mass., 1998).

³ Frank Tannenbaum, *Slave and Citizen: The Negro in the Americas* (New York, 1946); Stanley M. Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959).

⁴ In a way, a book like Berlin's combines both approaches, since he compares the evolution of racial slavery in four distinct regions of North America and also conveys a vivid picture of transatlantic connections over a period of two centuries. Like most historians, Berlin believes that we can use the general terms "slaves" and "slavery" despite the enormous variations and differences in the status, condition, and treatment of people classified as slaves. The now-classic works for defining the meaning

During the past thirty years, our understanding of American slavery has been extraordinarily enriched by numerous studies that fall in the first category of rigorous and sustained comparison. One thinks particularly of the work of Carl Degler comparing slavery and race relations in Brazil and the United States; George M. Fredrickson's two volumes on white supremacy and its consequences in the United States and South Africa; and Peter Kolchin's comparison and analysis of American slavery and Russian serfdom, a project that greatly broadened and enriched his subsequent survey of American slavery from 1619 to 1877.⁵ Mention should also be made of more specialized studies, such as those by Shearer Davis Bowman on U.S. planters and Prussian Junkers, by Eugene D. Genovese and Michael Craton on slave rebellions, and by Richard S. Dunn on two specific plantations in Virginia and Jamaica.⁶ While the comparative method *can* lead to mechanical listings of similarities and differences, it would clearly be useful to have more comparative studies on such specific subjects as domestic servants, slave artisans, and slaves in urban and manufacturing jobs. Peter Kolchin has candidly pointed to the severe problems comparative history faces, problems that help to explain the somewhat limited number of such full-length studies;⁷ yet I think that the cumulative benefit of comparative work can be seen in the global awareness of such historians as Thomas Holt, when writing on Jamaica;⁸ Rebecca Scott, when writing on Brazil and Cuba;⁹ Frederick Cooper, when writing on East Africa;¹⁰ and Seymour Drescher, when writing on British abolitionism and other subjects¹¹—to

of slavery are Orlando Patterson's *Slavery and Social Death: A Comparative Study* (Cambridge, Mass., 1982) and *Freedom in the Making of Western Culture* (New York, 1991). For a discussion of slavery as the ultimate limit in dehumanization, see my essay, "At the Heart of Slavery," *New York Review of Books* (October 17, 1996): 51–54 (a slightly revised version appears as "Introduction: The Problem of Slavery," in *A Historical Guide to World Slavery*, Seymour Drescher and Stanley L. Engerman, eds. [New York, 1998], ix–xviii).

⁵ Carl N. Degler, *Neither Black nor White: Slavery and Race Relations in Brazil and the United States* (New York, 1971); George M. Fredrickson, *White Supremacy: A Comparative Study in American and South African History* (New York, 1981); and *Black Liberation: A Comparative History of Black Ideologies in the United States and South Africa* (New York, 1995); Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987); and *American Slavery, 1619–1877* (New York, 1993).

⁶ Shearer Davis Bowman, *Masters and Lords: Mid-Nineteenth Century U.S. Planters and Prussian Junkers* (New York, 1993); Eugene D. Genovese, *From Rebellion to Revolution: Afro-American Slave Revolts in the Making of the Modern World* (Baton Rouge, La., 1979); Michael Craton, *Testing the Chains: Resistance to Slavery in the British West Indies* (Ithaca, N.Y., 1982); Richard S. Dunn, "A Tale of Two Plantations: Slave Life at Mesopotamia in Jamaica and Mount Airy in Virginia, 1799–1828," *William and Mary Quarterly*, 3d ser., 39 (January 1977): 32–65.

⁷ Peter Kolchin, "The Comparative Approach to the Study of Slavery: Problems and Prospects," paper given at the 1996 conference of the Ecole des Hautes Etudes en Sciences Sociales (EHESS).

⁸ Thomas C. Holt, *The Problem of Freedom: Race, Labor, and Politics in Jamaica and Britain, 1832–1938* (Baltimore, 1992).

⁹ Rebecca J. Scott, *The Abolition of Slavery and the Aftermath of Emancipation in Brazil* (Durham, N.C., 1988); *Slave Emancipation in Cuba: The Transition to Free Labor, 1860–1899* (Princeton, N.J., 1985).

¹⁰ Frederick Cooper, *From Slaves to Squatters: Plantation Labor and Agriculture in Zanzibar and Coastal Kenya, 1890–1925* (New Haven, Conn., 1980); *Decolonization and African Society: The Labor Question in French and British Africa* (Cambridge, 1996); *Plantation Slavery on the East Coast of Africa* (New Haven, 1977).

¹¹ Seymour Drescher, *Capitalism and Antislavery: British Mobilization in Comparative Perspective* (New York, 1987); Drescher and Christine Bolt, eds., *Anti-Slavery, Religion, and Reform: Essays in Memory of Roger Anstey* (Folkestone, 1980); Drescher, *Econocide: British Slavery in the Era of Abolition*

say nothing of the omnipresent economic historian Stanley L. Engerman, whose work on various forms of unfree labor could hardly be broader in perspective.¹²

But while careful, empirical comparison is indispensable, especially in alerting us to the importance of such matters as the demography and sex ratios of slave societies, the differences in slave communities, and the social implications of resident as opposed to absentee planters, much recent research has also underscored the importance of “the Big Picture”—the interrelationships that constituted an Atlantic Slave System¹³ as well as the place of such racial slavery in the evolution of the Western and modern worlds.

Gordon S. Wood, in a recent essay applauding the cosmopolitanism of American historians, has exclaimed that “[h]istorians of the United States no longer confine themselves to the nation’s borders; they now increasingly see the past of the United States as part of the larger history of the Atlantic world, if not of the entire globe.” He goes on to say: “Subjects such as the history of the slave trade, slavery, and African-American assimilation can no longer be understood within the confines of what became the continental United States. We now have to range from villages along the Gold Coast of Africa to the Cape Verde islands to Curaçao, Martinique, and Barbados to New Orleans.”¹⁴ I wish I could be as optimistic on these points as Wood, since I have been advocating such an approach to American slavery for nearly forty years and am still disheartened by the minimal effect that the vast scholarship on slavery has had on the job market and the course descriptions in most college catalogs. But perhaps this is a promising time to propose making New World slavery and abolition a unified, multinational subject for high school and college curricula as well as for cooperative research. I can think of no better window on the issues of power and exploitation, on outsiders and insiders, on the construction of race, on the expansion of the Euro-American West, on the early stages of consumer-driven economies, and on the promise and limitations of social reform.

(Pittsburgh, 1977); Drescher and Frank McGlynn, eds., *The Meaning of Freedom: Economics, Politics, and Culture after Slavery* (Pittsburgh, 1992).

¹² Stanley L. Engerman and Joseph E. Inikori, eds., *The Atlantic Slave Trade: Effects on Economies, Societies, and Peoples in Africa, the Americas, and Europe* (Durham, N.C., 1992); Engerman and Barbara L. Solow, eds., *British Capitalism and Caribbean Slavery: The Legacy of Eric Williams* (Cambridge, 1987); Engerman and Drescher, *Historical Guide to World Slavery*; Engerman and Robert L. Paquette, eds., *The Lesser Antilles in the Age of European Expansion* (Gainesville, Fla., 1996); Engerman and Eugene D. Genovese, eds., *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, N.J., 1975); Engerman and Robert W. Fogel, eds., *Without Consent or Contract: The Rise and Fall of American Slavery*; *Technical Papers* (New York, 1992); Engerman and Robert W. Fogel, *Time on the Cross: The Economics of American Negro Slavery* (Boston, 1974).

¹³ For a variety of reasons, I favor using the capitalized phrase, Atlantic Slave System, while recognizing that African slaves were transported at an early stage to such Pacific colonies as Peru and Chile, and that Mauritius and Réunion, in the Indian Ocean, came to resemble the slave plantation colonies of the Caribbean and were similarly impacted by Britain’s emancipation law of 1834. “New World slavery” is also a useful term but tends to close off the crucial initiative of Europe and Africa. An increasing number of studies underscore the interconnections of the Atlantic world and what Ira Berlin terms the “Atlantic creoles” who might have spent time in Lisbon, Seville, Madeira, São Tomé, Brazil, Barbados, Virginia, or New York. Eventually, the Atlantic Slave System did reach across the Pacific and was partially replaced by a Pacific labor system that included Hawaii and the Philippines and that drew on “coolie” labor from India, China, and other parts of Asia.

¹⁴ Gordon S. Wood, “Doing the Continental,” *New York Review of Books* (November 20, 1997): 51.

In fact, there is now much support for the view that the history of the United States—indeed, the history of the entire New World—has been dominated by the theme of slavery and freedom.¹⁵ In the 320 years from 1500 to 1820, every European immigrant who arrived in the New World was matched by at least two African slaves. It was African slaves and their descendants who furnished the basic labor power that created dynamic New World economies and the first international mass markets for such consumer goods as sugar, rice, tobacco, dyestuffs, and cotton.¹⁶ Yet, from the outset, the New World was seen by many, like the biblical Promised Land, as a space for new beginnings, for possibilities that would break free from the coercive bondage to the past. Paradoxically, the debasement of millions of workers to a supposedly bestial condition of repetitive time appeared to liberate other human beings to take control of their destiny, to “remake” themselves. This profound contradiction lay close to the core of the self-presentation of the new United States, which was “conceived in liberty” but based on slave labor, dedicated to certain “propositions” or principles, such as “all men are created equal,” but no less committed to compromises that protected the autonomy and political power of men who owned human property.

America’s bloodiest and most destructive war was perceived as an “apocalypse” and as a divine retribution precisely because it dealt with the consequences of what James Madison and other Founders had called “the original sin” of the African slave trade. The Civil War emancipated the largest population of slaves in the New World and exerted an indirect influence on the fate of slaves in Cuba and Brazil as well. Yet the Age of Emancipation, beginning with the Haitian Revolution of 1791 to 1804 and ending with Brazilian emancipation in 1888, simply shifted the problem of slavery and freedom to a new level. As the Western Hemisphere enters the twenty-first century, the legacy of its founding paradox is still apparent from Canada to Chile. The descendants of its eleven or twelve million involuntary African immigrants still carry both remnants of the rich and potentially rebellious culture that defied oppression and a stigma of slavishness kept alive by racism, poverty, and constricted opportunities.

The heritage of bondage seems as impervious to simple answers as was slavery itself. While we are the beneficiaries of genuine historical progress, we must guard against the “generational chauvinism” that assumes we are wiser or more virtuous than the European, African, and American creators of the highly complex Atlantic Slave system. When we realize that for many generations historians and schoolbooks virtually ignored the central problem of slavery and freedom, or consigned an extremely limited and romanticized version of the subject to a marginal subfield, “The History of the South,” we begin to recognize the importance of recovering “the Big Picture”—a comprehensive view of the Atlantic Slave System as a whole.¹⁷

¹⁵ See David Brion Davis and Steven Mintz, *The Boisterous Sea of Liberty: A Documentary History of America from the Discovery through the Civil War* (New York, 1998).

¹⁶ For a recent and especially thought-provoking view, see Richard Lyman Bushman, “Markets and Composite Farms in Early America,” *William and Mary Quarterly*, 3d ser., 55 (July 1998): 351–74.

¹⁷ In 1936, an immensely popular survey of American history, written by W. E. Woodward (no relation of C. Vann Woodward), contained the following summation of Ulrich B. Phillips’s far more scholarly work of 1918: “The slave system . . . did incalculable harm to the white people of the South, and benefited nobody but the negro, in that it served as a vast training school for African savages. Though the regime of the slave plantations was strict it was, on the whole, a kindly one by comparison

As the eminent scientist Freeman Dyson has pointed out, "the understanding of the component parts of a composite system is impossible without an understanding of the behavior of the system as a whole . . . The progress of science requires the growth of understanding in both directions, downward from the whole to the parts and upward from the parts to the whole."¹⁸ Although I have never considered history a science, I am convinced that the Big Picture is indispensable as a first step toward coming to terms with the nature and workings of historical evil, while avoiding the pitfalls of demonizing special groups or dividing history into paranoid struggles between the children of light and the children of darkness—struggles like those between medieval Christians and Muslims that shaped the first foundations for ethnic slavery in the Mediterranean and Atlantic.

These were the premises that led me in 1995 to develop a lecture course, analogous in some ways to a survey of world history, on what the titles of Robin Blackburn's recent books describe as *The Making of New World Slavery* and *The Overthrow of Colonial Slavery*.¹⁹ Necessarily, such a course must be highly selective, and I should add that my course for Yale undergraduates was the outgrowth of an earlier and even briefer summer course for New York City teachers, which I have continued to give for six years. But I think it can be argued that intelligent students and teachers can read what Aristotle and the Stoics wrote about slavery, and grasp the relevance of such arguments to later times, without knowing much about the history of ancient Greece. Similarly, one can read Alan Watson's classic work *Roman Slave Law*, especially as an introduction to Thomas D. Morris's *Southern Slavery and the Law, 1619–1860*,²⁰ without knowing the names of the Roman emperors or whether there were one or two versions of Gratian's crucial twelfth-century *Decretum*, which founded the science of canon law.

This approach obviously runs against the increasing pressures for temporal and geographic specialization. The historian or teacher who scans multiple centuries and several continents, even with selective post-hole digging, risks almost inevitable errors as well as superficiality. Some form of team teaching and cooperative research might well be preferable for such Big Picture subjects. All the same, individual teachers throughout the country have long been required to teach surveys of world history, Western Civilization, or American history from pre-Columbian times to the present. The rich outpouring of monographic literature during the past forty years, illustrated and made accessible by such reference works as the American Historical Association's most recent two-volume *Guide to Histor-*

with what the imported slave had experienced in his own land. It taught him discipline, cleanliness and a conception of moral standards." *A New American History* (New York, 1936), 412. When I began graduate work at Harvard in 1951, Phillips's *American Negro Slavery: A Survey of the Supply, Employment and Control of Negro Labor as Determined by the Plantation Régime* (1918; rpt. edn., Gloucester, Mass., 1959), was the standard scholarly work on the subject.

¹⁸ Freeman Dyson, "The Scientist as Rebel," *New York Review of Books* (May 25, 1995): 32. This essay originally appeared in John Cornwell, ed., *Nature's Imagination: The Frontiers of Scientific Vision* (New York, 1995), 1–11.

¹⁹ Robin Blackburn, *The Making of New World Slavery, from the Baroque to the Modern, 1492–1800* (London, 1997); *The Overthrow of Colonial Slavery, 1776–1848* (London, 1988).

²⁰ Alan Watson, *Roman Slave Law* (Baltimore, 1987); Thomas D. Morris, *Southern Slavery and the Law, 1619–1860* (Chapel Hill, N.C., 1996).

ical Literature,²¹ provides firm grounding for various experiments in synthesis. And as most of us learned in our earliest apprenticeship years, colleagues and reference librarians, now reinforced by the Internet, can be essential in guiding us to the information we need. Above all, inquiries into the distant origins and significance of New World slavery can teach us new ways of viewing the end of European serfdom, the Renaissance, the Reformation, the emergence of European market economies, the spread of Islam in Africa—in short, viewing unsuspected interrelationships that transform our sense of the past.

I WOULD NOW LIKE TO ILLUSTRATE MY PROPOSAL by describing a number of specific subjects that are touched on, at least, at the beginning of my evolving course, subjects that may convey broader perspectives for viewing New World slavery. Such an immense field of study must have flexible and changing boundaries, but some coherence is provided, I would argue, by focusing on the most extreme and systematic form of personal domination, dishonor, dehumanization, and economic exploitation, a form of domination and exploitation that became a model, in the eyes of successive generations of liberationists, for all Western and white male imperialism.

I am aware, of course, that regardless of law or theory, a slave's actual status could historically vary along a broad spectrum of rights, powers, and protections. There was much diversity both within and among slave systems, which is one of the things that makes the subject so fascinating. Some of the privileged "Atlantic creole" slaves in the seventeenth-century Chesapeake and in Carolina clearly possessed more *de facto* freedom and range of choice than did the later Chinese indentured servants who shoveled guano, or seafowl excrement, off the coast of Peru. In a truly brilliant stroke, Orlando Patterson chose to describe and classify the most "elite slaves of the Islamic states and empires, and the palatine eunuchs of Byzantium and imperial China" as "the ultimate slaves." Whatever power and privilege such slaves and freedmen possessed, "it existed solely at the whim, feeble-mindedness, or design of the master."²² As Patterson demonstrates in his discussion of manumission, even a liberated slave accepts, to some degree, the conceptual structure of slavery and emancipation. The many metaphorical uses of "slavery" and "enslavement" point to the remarkable stability and continuity of the *concept* of total subordination, vulnerability, and animalization.

This theme is illustrated in some ways by my first specific subject, the place of slavery in the Bible and its interpretations. This became an absolutely central issue for American abolitionists and proslavery theorists, even though modern historians have not given the matter the attention it deserves. Michael Walzer has shown that the narrative of the ancient Hebrews' escape from Egyptian bondage inspired not only American slaves but various revolutionary movements through history.²³ Yet the Israelites' liberation from physical bondage required an uncommitted bondage

²¹ *The American Historical Association's Guide to Historical Literature*, Mary Beth Norton, gen. ed., 2 vols., 3d edn. (New York, 1995).

²² Patterson, *Slavery and Social Death*, 299–333, 307.

²³ Michael Walzer, *Exodus and Revolution* (New York, 1985).

to God, just as the later Christians' liberation from sin required an uncommitted bondage to Christ. One of the central, authoritative messages of the Judeo-Christian Bible is that all human beings need *control*, a kind of subordination, governance, and dependence similar to that of slavery. Given the Augustinian view of original sin, this outlook could and did justify physical slavery as a lesser of evils. Yet Jews were exhorted to remember "that thou wast a bondman in the land of Egypt, and the Lord thy God redeemed thee," and Jesus repeated Isaiah's words about being sent "to proclaim liberty to the captives, and the opening of the prison to them that are bound."²⁴ This tension between various forms of liberation and bondage pervades the Bible. If both Jews and Christians felt authorized to enslave outsiders—"Canaanites," "the children of strangers," or heathen and infidels—more difficult questions arose when the outsiders chose to or were permitted to become insiders.²⁵ This is a complex issue, embodied in debates over the effects of baptism (such as the debates over baptizing slaves at the Synod of Dort in 1618), on which we need more comparative research.

My second suggested subject, the ending of slavery and a growing commitment to freedom in northwestern Europe, was long interpreted as a result of Christianization and the ways a shared religious faith gradually undermined the authority of masters. Even though this much-debated subject has recently led to highly relevant monographs, such as Ruth Mazo Karras's *Slavery and Society in Medieval Scandinavia*,²⁶ only a few historians, such as William D. Phillips, Jr., and Robin Blackburn, have tried to make connections between gradual freedom in Europe and the supposed rebirth of slavery in the New World.²⁷ Yet when we stand back and take a wide-angled view, it is an astonishing paradox that the first nations in the world to free themselves of chattel slavery—such nations as England, France, Holland, and even the Scandinavian states—became leaders during the seventeenth and eighteenth centuries in supporting plantation colonies based on African slave labor. Here we have the beginnings of primitive "Mason-Dixon" lines, now drawn somewhere in the Atlantic, separating free soil master-states from tainted slave soil dependencies.²⁸

Historians of America cannot be expected to become medievalists, but it can be useful to know some of the conflicting explanations for the disappearance of slavery and serfdom from western Europe, as opposed to the increasing debasement of workers in eastern Europe. For example, Karras, in a seeming reversal of the

²⁴ Isaiah 61: 1; Luke 4: 16–18.

²⁵ As Benjamin Braude has recently shown, historians have long misinterpreted the biblical "Curse of Canaan" and the historical meaning of Ham. "The Sons of Noah and the Construction of Ethnic and Geographical Identities in the Medieval and Early Modern Periods," *William and Mary Quarterly*, 3d ser., 54 (January 1997): 103–42. We should learn much more from Braude's forthcoming book on the subject.

²⁶ Ruth Mazo Karras, *Slavery and Society in Medieval Scandinavia* (New Haven, Conn., 1988).

²⁷ William D. Phillips, Jr., *Slavery from Roman Times to the Early Transatlantic Trade* (Minneapolis, 1985); Blackburn, *Making of New World Slavery*. See my own *Slavery and Human Progress* (New York, 1984); and *The Problem of Slavery in Western Culture* (New York, 1988; orig. pub. 1966). For the European elites' stereotypes of medieval peasants, which strikingly anticipate early modern racist views of African slaves, see Paul Freedman, *Images of the Medieval Peasant* (Stanford, Calif., 1999).

²⁸ In a pathbreaking exercise in counterfactual history, David Eltis has argued that Western Europeans would have populated the New World with white European slaves if cultural inhibitions had not checked pure economic interest. "Europeans and the Rise and Fall of African Slavery in the Americas: An Interpretation," *AHR* 98 (December 1993): 1399–1423.

Edmund Morgan thesis, suggests that the ending of slavery in Scandinavia was the result of growing *inequalities* among the nonslave population. For many centuries, she writes, Scandinavian society was divided by a single great chasm: slaves and nonslaves. While much inequality prevailed within the latter group, the poorest peasant could take pride in not being a slave. But as a result of population growth and limited land, the mass of peasants by the thirteenth century were forced into tenancy, while the rich and powerful became obsessed with hereditary rank and nobility. The word “*fraels*,” meaning “free,” which had once been applied to all nonslaves, now became restricted to the privileged nobility. According to Karras, this growing social hierarchy removed the ideological function that slavery had long served: “Once not being a slave no longer automatically meant that one was fully free and independent, the institution of slavery became obsolete.” Although one must take account of the wholly different economies in the New World, Karras’s argument may shed some light on the great American paradox of slavery and democracy marching ahead to the same beat.²⁹

In contrast to Portugal and Spain, which began absorbing shipments of African slaves in the mid-fifteenth century, the supposedly free soil or free air of England and France became inhospitable to colonial planters who returned home with a retinue of black slaves. Part of the difference was probably due to a stronger inclination toward racism in the northern regions, as indicated by attempts to expel all blacks in late Elizabethan England.³⁰ Yet as Sue Peabody has shown in her fascinating book “*There Are No Slaves in France*,” racism mixed in eighteenth-century France with a strong ideological aversion to slavery, a deep determination to prevent colonial bondage from contaminating a “free” but authoritarian and highly stratified nation.³¹ A comparative study might show how the French courts that freed hundreds of slaves brought to France foreshadowed the later personal liberty laws in the American North, to say nothing of Abraham Lincoln’s fear of a “Second” Dred Scott decision that would legalize slaveholding in the North.

In certain contexts, the institution of slavery represented a deadly threat to historically won freedoms that seemed to lie at the heart of English, French, and American identity. Yet despite slavery’s seemingly retrograde character, which encouraged many writers in the eighteenth and nineteenth centuries to interpret it as an anachronistic innovation, there were strong sequential links between the Atlantic Slave System and the Italian Renaissance, then the Spanish world of El Greco and Cervantes, then the Dutch world of Frans Hals, Rembrandt, Vermeer, and Hobbema, then the French *ancien régime* of Montesquieu, Voltaire, Diderot,

²⁹ Karras, *Slavery and Society in Medieval Scandinavia*, 156–66. Edmund S. Morgan’s famous thesis is presented in his *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975).

³⁰ Alden T. Vaughan and Virginia Mason Vaughan, “Before *Othello*: Representations of Sub-Saharan Africans,” *William and Mary Quarterly*, 3d ser., 54 (January 1997): 42. As James H. Sweet demonstrates in this same volume titled “Constructing Race: Differentiating Peoples in the Early Modern World” (Sweet, “The Iberian Roots of American Racist Thought,” 143–66), the Iberian Peninsula was hardly free of racism; indeed, in many respects, it was the seedbed of an anti-black racism that had long been associated with the enslavement of sub-Saharan Africans and with the earlier expansion of Islam (see my *Slavery and Human Progress*, 32–51).

³¹ Sue Peabody, “*There Are No Slaves in France*”: *The Political Culture of Race and Slavery in the Ancien Régime* (New York, 1996).

Rousseau, Laplace, and Lavoisier, and above all, the England of Locke, Newton, Defoe, Swift, Watt, Johnson, Hume, Hogarth, Gibbon, and Blake. I do not mean to imply that these famous figures had any direct ties with New World slavery, although I might mention that both Locke and Voltaire, like thousands of ordinary people, drew income from the African slave trade. My point, which still pertains to the “Mason-Dixon” lines between center and periphery, concerns the curious correlation between national participation in the evolving slave trade and slave system, on the one hand, and cultural achievement, on the other. From Renaissance Florence and Genoa to eighteenth-century England, each region’s maximum involvement with African slaves coincided with a time of cultural creativity that was at least remotely associated with economic and political power.

Mention of Renaissance Italian city-states brings me to a third subject, the often ignored extension of slave trading and sugar cultivation from the Mediterranean to such Atlantic islands as Maderia and São Tomé, off the coast of West Africa, which then became the models for the spread of racial slavery and sugar plantations to the Caribbean and Brazil. As Charles Verlinden showed long ago, medieval entrepreneurs in Venice and Genoa took the lead in establishing slaveholding colonies in the eastern Mediterranean and in setting up entrepôts on the coasts of the Black Sea and Sea of Azov for the purchase of large numbers of slaves.³² By the 1380s, the slave populations from Cyprus to Catalonia, in the western Mediterranean, were largely composed of Circassians, Georgians, Armenians, Tatars, Bulgarians, and other peoples transported by Italian slave ships from the Black Sea. Venetian and Genoese merchants were also pioneers in supplying expensive copper boilers and hydraulic mills for the production of sugar in Cyprus, Crete, and Sicily. There is still some controversy and uncertainty over the status of Mediterranean laborers used in the cultivation of sugar, which was still a luxury only the affluent could afford, often to sweeten their drinks. But as the European demand for sugar continued to rise with the monetary expansion and population recovery of the second half of the fifteenth century, sugar and *racial* slavery became slowly intermeshed in Madeira, the Canary Islands, and São Tomé.³³

While American historians are familiar with the role of such Italians as Columbus, John Cabot, Alvise da Cadamosto, and Amerigo Vespucci in the revolutionary global expansion of Europe, far less attention has been paid to the Italian merchants and bankers who provided the capital, technology, and sailors that enabled the Portuguese to establish an African and Asian commercial empire, to colonize Brazil, and to build the first foundations of the Atlantic Slave System. The Marchionni family, of Renaissance Florence, gives us a glimpse of this Mediterranean-to-Atlantic continuity. The Marchionnis were long engaged in the

³² Charles Verlinden, *L’esclavage dans l’Europe médiévale*, Vol. 1: *Péninsule Ibérique, France* (Bruges, 1955); Vol. 2: *Italie, Colonies italiennes du Levant, Levant latin, Empire bysantin* (Gent, 1977); *The Beginnings of Modern Colonization: Eleven Essays with an Introduction*, Yvonne Freccero, trans. (Ithaca, N.Y., 1970).

³³ Even in Sicily as early as 1430, notaries began to identify *scilavi*—the Latin term that originally associated slaves with people of Slavic origin—as *scilavi nigri* (literally “black Slavs”), a category separate from “black Saracens.” As Verlinden notes, “as in Naples, slavery in Sicily at the end of the fifteenth century was preeminently black slavery.” He goes on to conclude that the Mediterranean had developed an “American” form of slavery several decades before America was discovered. Quoted in Davis, *Slavery and Human Progress*, 56, see also 36–57, 332.

Black Sea slave trade. Kaffa, their base in the Crimea, resembled in many respects the forts and trading-posts Europeans would later establish along the West African coast. But in 1453, the Ottoman Turks captured Constantinople and soon diverted the flow of Black Sea and Balkan captives to Islamic markets. Ottoman expansion threatened the Italian colonies in the eastern Mediterranean and also diminished Europe's supply of both sugar and slaves. It was against this background, which included a growing number of black slaves transported north by Arab caravans and sold in such plantation regions as Sicily and Majorca, that young Bartolommeo Marchionni moved in 1470 from Florence to join the growing community of Florentines in Lisbon.

Italian immigrants had done much to build Prince Henry's fleet, which had opened up commercial relations with West Africa. When Bartolommeo Marchionni arrived in Lisbon, Italian merchants were already transporting sugar to Antwerp and even England. As early as 1317, a Portuguese king had promised a Genoese merchant one-fifth the value of all slaves brought back from the Moroccan coast, a venture that proved to be premature and unsuccessful. But Bartolommeo Marchionni soon owned sugar plantations in Madeira. The Portuguese king also sold him the right to trade in slaves from Guinea. According to Hugh Thomas, Marchionni then became "a monopoly trader in slaves from the Benin River in the 1490s, carrying captives not only to Portugal and Madeira but also to Elmina, on the Gold Coast, where he sold them to African merchants for gold." This friend of the Medicis and Amerigo Vespucci also sent African slaves back to Pisa and Tuscany.³⁴ Thanks in part to the skills and knowledge of such Florentine and Genoese entrepreneurs, when Columbus first entered the Caribbean, thousands of black slaves in the Old World were already producing sugar for a European market.

While knowledge of these continuities and transformations is essential for any understanding of the Big Picture, we must always be alert to the contingency of most historical events and be on guard against a deterministic or teleological view of New World slavery. Various historians have pointed out that no European nation considered African-derived slaves as an intrinsic part of its plans for New World colonization, and much attention has been given to the belated and hesitant acceptance of black slavery in the Spanish Caribbean and especially in the English Chesapeake.

Therefore, a fourth subject, which I would divide if I had space into several large and distinct parts, concerns the events and elements that converged to create the Atlantic Slave System and that thereby give added significance to the earlier points I have tried to make. One might begin with the Portuguese interaction with sub-Saharan Africans and the material John Thornton discusses in *Africa and Africans in the Making of the Atlantic World, 1400–1680*, to say nothing of Stuart B. Schwartz's *Sugar Plantations in the Formation of Brazilian Society*, and Joseph Miller's *Way of Death*.³⁵ As one takes account of the strong and active role taken by various African peoples, new questions arise concerning the Africans' desire for

³⁴ Hugh Thomas, *The Slave Trade: The Story of the Atlantic Slave Trade, 1440–1870* (New York, 1997), 10–11, 13–14, 42, 84–85, 94–96, 105, 111, 269, 291–92, 791; Verlinden, *L'esclavage dans l'Europe médiévale*, 2: 235, 377.

³⁵ John Thornton, *Africa and Africans in the Making of the Atlantic World, 1400–1680* (Cambridge, 1992); Stuart B. Schwartz, *Sugar Plantations in the Formation of Brazilian Society: Bahia, 1550–1835*

Asian and European commodities, the nature and extent of resistance, the place and significance of Portuguese *lançados* (castoffs, settlers in Africa), including traders of mixed Portuguese and African ancestry, and the failure of various European and African expressions of protest. If the African slave trade was connected with Hungarian copper mines and with rich merchant capitalist families such as the Fuggers and Welsers of Augsburg, were there comparable networks of profiteering in Africa? The recognition of greater African agency in creating and sustaining the slave trade and in contributing to a larger Atlantic and maritime culture necessitates at least some knowledge of the current findings and controversies in African history.³⁶

A fifth and no less immense subject concerns the cultural construction of race and the divergent ways in which Europeans perceived sub-Saharan Africans and Native Americans. On this issue, a broader perspective first moves us backward in time to the early debates between Carl Degler, the Handlins, and Winthrop Jordan over the development of racism in colonial Virginia.³⁷ But as one can see from the early volumes of *The Image of the Black in Western Art* and from the special 1997 issue of the *William and Mary Quarterly* devoted to "Constructing Race,"³⁸ the evidence is still mixed and can point to quite different conclusions.

Some historians have presented a generally negative European tradition regarding Africa that extends back through Leo Africanus, various editions of *Mandeville's Travels*, Benjamin of Tudela, and Isidore of Seville, to such classical writers as Pliny the Elder and Herodotus. The naked Native Americans, on the other hand, living in apparent freedom and innocence, awakened memories of terrestrial paradise and the Golden Age described by the ancients. Of course, Iberians also created a literature on Indian cannibalism and human sacrifice, and Columbus himself set the precedent for enslaving the natives of the New World. The Spanish and Portuguese earlier slaughter and enslavement of the Berber-like Canary Islanders has quite rightly been seen as a prelude to the slaughter and enslavement of Caribbean, Mexican, Central American, and Brazilian Indians.

Nevertheless, from the Spanish government's attempts to outlaw the enslavement of Indians to the startling contrast between early English perceptions of dignified, aristocratic Indians and "brutish," "monstrous" Africans, Europeans drew distinctions between the two peoples that were further magnified by the appalling mortality of Indians and their seeming weaknesses as agricultural workers, when compared with African slaves. One must remember that the Portuguese and Spaniards had long been familiar with black slaves, at least 100,000 of whom had

(Cambridge, 1985); Joseph C. Miller, *Way of Death: Merchant Capitalism and the Angolan Slave Trade, 1730-1830* (Madison, Wis., 1988).

³⁶ See especially Joseph C. Miller's presidential address, "History and Africa/Africa and History," *AHR* 104 (February 1999): 1-32.

³⁷ Oscar Handlin and Mary F. Handlin, "Origins of the Southern Labor System," *William and Mary Quarterly*, 3d ser., 7 (1950): 199-222; Carl N. Degler, "Slavery and the Genesis of American Race Prejudice," *Comparative Studies in Society and History* 2 (1959): 49-66; Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550-1812* (Chapel Hill, N.C., 1968).

³⁸ Ladislav Bugner, ed., *The Image of the Black in Western Art*, Vol. 1: *From the Pharaohs to the Fall of the Roman Empire* (New York, 1976); Vol. 2: *From the Early Christian Era to the "Age of Discovery"* (New York, 1979); *William and Mary Quarterly*, 3d ser., 51 (January 1997).

been shipped to Iberia in the half-century before Columbus's first voyage. Indeed, James H. Sweet argues that Iberian Christians took over and magnified Muslim stereotypes of black slaves and that this racism

was a necessary precondition for the system of human bondage that would develop in the Americas during the sixteenth century and beyond . . . The virulence of racism increased as economic imperatives fueled Africans' debasement, but the origins of racism preceded the emergence of capitalism by centuries . . . Biological assumptions that were familiar to a nineteenth-century Cuban slaveowner would have been recognizable to his fifteenth-century Spanish counterpart.³⁹

I am inclined to accept this judgment, as well as the arguments of Yosef Hayim Yerushalmi and Ronald Sanders about the importance of the Spaniards' obsession with "purity of blood," as a reaction to the assimilation of *conversos*, or New Christians, and Moriscos, who had Moorish ancestry.⁴⁰ It seems highly probable that during the fifteenth and sixteenth centuries the negative imagery first associated with European rustics, Jews, and Moriscos was projected upon the newly arrived African slaves, who could be perceived as the archetype of the subhuman. Yet we must also recognize a degree of European ambivalence and flexibility regarding sub-Saharan Africans in the pre-colonization period. Benjamin Braude, for example, has underscored the danger of assuming that medieval Europeans automatically associated the biblical Ham with Africa.⁴¹ The lack of any single or consistent cultural tradition concerning Africa meant that popes could look forward to Ethiopian Christian missions to Rome from Jerusalem or Ethiopia itself, that in scenes of the Nativity a non-kneeling Magus could be depicted with Negroid features, and that similar features could appear in northern European paintings and statues of St. Maurice, the Theban martyr who by the mid-thirteenth century had become a Germanic saint heading the conquest and Christianization of pagan Slavs and Magyars!⁴² My students have been quite frankly amazed when I have tried to illustrate these points with slides of medieval art.

These five broad subjects constitute only the introduction to a course that moves on to the Middle Passage and the way it was experienced; to the intrusion of the Dutch in Africa, Brazil, and the Caribbean; to comparisons of plantation slavery in Brazil, the British Caribbean, and North America; and to the impact of the American, French, and Haitian Revolutions. Even seventy-five-minute lectures must be highly selective and reinforced by extensive reading, and I am afraid that the course must presume that students have a fairly firm grounding in world history. But, as I suggested at the outset, this approach may be thought of as a different way of perceiving world history.

³⁹ Sweet, "Iberian Roots of American Racist Thought," 166.

⁴⁰ Yosef Hayim Yerushalmi, *Assimilation and Racial Anti-Semitism: The Iberian and the German Models* (New York, 1982); Ronald Sanders, *Lost Tribes and Promised Lands: The Origins of American Racism* (Boston, 1978), 17–73; Jerome Friedman, "Jewish Conversion, and the Spanish Pure Blood Laws and Reformation: A Revisionist View of Religious and Racial Anti-Semitism," *Sixteenth Century* 18 (1987): 3–29; Marc Shell, "Marranos (pigs); or, From Coexistence to Toleration," *Critical Inquiry* 17 (Winter 1991): 306–36. I am much indebted to Harold Brackman for calling my attention to most of these sources.

⁴¹ Braude, "Sons of Noah," 103–42.

⁴² See Bugner, *From the Early Christian Era to the "Age of Discovery,"* Part 2, 100–258.

After considering the Age of Revolution, I move on to a more extensive examination of slavery in the nineteenth-century South; to slave conspiracies and revolts including the *Amistad* and *Creole* cases,⁴³ which underscore the multinational aspects of the subject; to explanations of British abolitionism; and to white and black abolitionists in America. After then examining the iconography of race in the nineteenth century, I turn to what I term “the seriousness of proslavery arguments”; the politics of slavery in the United States; the Civil War and slave emancipation; emancipation in Cuba and Brazil, and the meaning of such New World liberations for later times, especially with a view to the persistence of massive forms of unfree labor and the relative lack of twentieth-century protest.⁴⁴

While it would take many pages to discuss the questions raised by these topics—and the teachers of such a course must find ways to counteract abstractions and re-create individual experience—we must above all stress the importance of international interconnections and influence, an issue that cries out for much further research. Thus historians focusing on England often forget that enemies can be particularly influential. The English adopted, after all, the Spanish word “Negro” and celebrated the outlaws like John Hawkins who succeeded in breaking the Portuguese monopoly and in transporting African slaves to the Spanish-American market. In actuality, England long profited from a close alliance with Portugal, and English merchants and sailors must have become familiar with black slaves in Madeira well before Columbus. (I might add that Columbus himself had a long residence in Madeira.) The multinational character of the slave trade is symbolized by the Dutch man-of-war that brought twenty blacks to Virginia in 1619.⁴⁵ Far more important were the Dutch refugees, following Portugal’s reconquest of Brazil, who taught the British in Barbados how to grow and mill sugar cane. It was Dutch expertise and naval power that opened the Caribbean to British and French colonization, and it was then English émigrés from Barbados who helped to found South Carolina.

These webs of interconnection, which extend to the influence of American Quakers on British abolitionism, the influence of British abolitionists on America, and of American abolitionists on Brazilian reformers, should finally give substance and light to the great overarching issues regarding slavery, capitalism, and modernity. As Peter Kolchin has trenchantly observed, slavery in the American South “was a bundle of contradictions”:

⁴³ In the *Amistad* case, a U.S. district court and then the Supreme Court freed a group of African captives who had been shipped to Cuba, contrary to Spanish law and treaties between Spain and England, even though the Africans had risen in revolt and killed the captain of the *Amistad*, a ship transporting them along the Cuban coast for sale, after which the ship had sailed north and had been seized by an American revenue cutter. The decisions outraged Southerners as well as the Spanish and Cubans. One year later, in 1842, slaves revolted on an American ship, the *Creole*, en route from Virginia to New Orleans. The slaves steered the *Creole* to the British island Nassau, where British authorities freed them. After the United States threatened England with war, an international tribunal ruled that Britain had been in the wrong and was obliged to pay \$100,000 to the American claimants.

⁴⁴ For a rather controversial examination of modern slavery and the relative lack of organized protest, see Kevin Bales, *Disposable People: New Slavery in the Global Economy* (Berkeley, Calif., 1999).

⁴⁵ See Engel Sluiter, “New Light on the ‘20. and Odd Negroes’ Arriving in Virginia, August 1619,” *William and Mary Quarterly*, 3d ser., 54 (1997): 396–98; John Thornton, “The African Experience of the ‘20. and Odd Negroes’ Arriving in Virginia in 1619,” *William and Mary Quarterly*, 3d ser., 55 (July 1998): 421–34.

Rooted in the lust for profits, it fostered a paternalistic ideology that denigrated crude materialism as a “Yankee” vice. Inextricably linked to the North—and the wider world—through international markets, it produced an intense attachment to section, state, and locality that belied the growing economic interdependence of the modern world. A great success story in terms of economic growth, it left the South seriously underdeveloped both economically and socially . . . Predicated upon denial of freedom to a substantial proportion of the population, it was defended by men who talked endlessly of their passionate commitment to “liberty.”⁴⁶

These contradictions reached their peak in postrevolutionary America, where, as Kolchin puts it, slavery could be thought of as the “dark underside of the American dream.” But in other parts of the New World, such as the Caribbean, the symptoms of economic and social underdevelopment were even more flagrant. The degenerate, disease-like effects of New World slavery were easy to account for in the days when Joseph Lowell Ragatz, Eric Williams, and other historians portrayed slave labor as a negative, unproductive force that reversed all human progress. But in the aftermath of “the Cliometric Revolution,” historians have not yet found a convincing way to reconcile slavery’s productivity and economic growth with evidence of paternalism, slave resistance, or social imbalance and decay. How could workers who were relatively free from market forces produce so much or drive such economic growth, especially when historians claim they were engaged in subtle forms of day-to-day resistance? I have seen no satisfactory answer to such questions, but suspect that the negotiating and bargaining between slaves and masters often led to compromises that actually aided productivity. Further, more attention needs to be given to the psychology and dynamics of nearly absolute power. Since the furious attacks on Stanley Elkins’s often misunderstood thesis, this is an academic turf on which few have dared to tread.⁴⁷

It is worth noting that Elkins, like Eric Williams, dealt with both slavery and abolitionism. Even though Roger Anstey, Seymour Drescher, Robin Blackburn, and a few others have continued this tradition, in general the specialists on abolition and the specialists on slavery and the slave trade live in entirely different worlds. This, it seems to me, is understandable but most unfortunate. There are any number of subjects that might serve to bring specialists from the two groups together in a way that would certainly broaden our perspectives.⁴⁸ For example, in October 1999, Yale held a small conference on the internal or “domestic” slave trades in the United States, Brazil, and the British West Indies.⁴⁹ Each of these involuntary migrations of slaves attracted much attention from abolitionists, who, in the British case, succeeded in drastically limiting the flow of coerced labor from the older to such newer colonies as Trinidad and Guyana. In the United States, the issue involved the possible constitutional power of Congress to regulate interstate trade.

While historians have given some attention to comparing post-emancipation

⁴⁶ Kolchin, *American Slavery*, 170.

⁴⁷ Elkins, *Slavery: A Problem in American Institutional and Intellectual Life*. Like many books with a complex “thesis,” Elkins’s points have often been oversimplified and even caricatured.

⁴⁸ This will be one of the goals of Yale’s new Gilder Lehrman Center for the Study of Slavery, Resistance, and Abolition.

⁴⁹ The dates were October 22–24.

societies, a subject that needs much more scrutiny, there is a final question that might be raised in our search for broader perspectives: what effect, if any, have the great nineteenth-century slave emancipations had on twentieth-century forms of unfree labor? Millions of people celebrated in an almost ecstatic, millennial way the successive liberations of New World slaves. As one reads the speeches and sermons, it appears that all humans will be free forevermore. Yet the twentieth century has clearly witnessed more slavery than all the preceding centuries combined. I have in mind not only the tens of millions of men, women, and children who were subjected to state servitude by Nazi Germany, Communist Russia, Communist China, and smaller totalitarian states. Or the Southern blacks in chain gangs and the often coerced migratory farm workers.⁵⁰ In a sense, the multinational Atlantic Slave System can be seen as the first stepping stone toward the multinational corporations that today employ millions of virtual slaves in various construction and production projects in Asia, Africa, and Latin America.⁵¹ One interesting point is why we have had so few counterparts in the past century to the millions of agitators and petition-signers who did so much to overthrow chattel slavery from 1788 to 1888. Did their triumph pass on a legacy that there was nothing further to do? Or is the evil of bondage limited to individual ownership? Or have we simply adopted a more complacent and cynical worldview? Perhaps more courses and books dealing with the Big Picture will give us an answer.

⁵⁰ See, for example, David M. Oshinsky, *Worse Than Slavery: Parchman Farm and the Ordeal of Jim Crow Justice* (New York, 1996).

⁵¹ Bales, *Disposable People*. In evaluating modern slavery, much obviously depends on definition; the individual ownership of human beings has largely given way to what seems like enslavement to a state, prison, or corporate entity (of course, our Thirteenth Amendment *allows* slavery or involuntary servitude "as a punishment for crime whereof the party shall have been duly convicted"). Kevin Bales defines slavery as "*a person held by violence or the threat of violence for economic exploitation*" (p. 280, italics in original). Orlando Patterson has pointed to examples of pre-modern slavery that had nothing to do with economic exploitation, and gives the following "preliminary definition of slavery on the level of personal relations: *slavery is the permanent, violent domination of naturally alienated and generally dishonored persons*" (Patterson, *Slavery and Social Death*, 13, italics in original).

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The Big Picture: A Comment on David Brion Davis's
"Looking at Slavery from Broader Perspectives"

PETER KOLCHIN

LET ME BEGIN BY SAYING that I applaud David Brion Davis's commitment to getting away from a parochial view of American slavery by looking at what he terms "the Big Picture." I am enthusiastic about his evolving course on the rise, development, and fall of the Atlantic slave systems; indeed, I wish that I could take this course. In his essay, he has, with characteristic clarity, shown us how to look at slavery from a broader perspective and at the same time has skillfully synthesized much of the recent historiography on slavery in the modern world.

I would like to raise two interrelated questions about the approach that Davis has outlined for us. First, I wonder about the precise relationship between what he proposes and *comparative* history. Davis suggests that there are two distinct "ways of looking at slavery from broader perspectives": one of these is the "comparative approach" and the other is his "global or multinational view of the origins, development, and abolition of racial slavery in the New World." In fact, of course, there is considerable overlap between these two approaches, and in his "broader perspective" Davis embraces a good deal of what is usually subsumed under the rubric of "comparative" history.¹ I would like to see him address more explicitly the relationship between his multinational approach and comparison, and the extent to which he sees the former as incorporating the latter.

Comparative history can serve a number of different functions, but in engaging in historical comparison, scholars usually aim to do at least one of three things: create an awareness of how a particular case fits into a broader pattern, thereby reducing historical parochialism; test hypotheses and form historical generalizations; or identify and explain significant differences.² Davis's multinational ap-

¹ This also is true of his monumental books on the "problem of slavery," in which he provides very much the kind of broad view that he advocates in his essay. See David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966); *The Problem of Slavery in the Age of Revolution, 1770-1823* (Ithaca, 1975); and *Slavery and Human Progress* (New York, 1984).

² On the nature and methodology of comparative history, see Raymond Grew, "The Case for Comparing Histories," *AHR* 85 (October 1980): 763-82; George M. Fredrickson, "Comparative History," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writings in the United States* (Ithaca, N.Y., 1980), 457-73; and Fredrickson, "From Exceptionalism to Variability: Recent Developments in Cross-National Comparative History," *Journal of American History* 82 (September 1995): 587-604. On applying comparison to American history, and especially to the American South and American slavery, see Peter Kolchin, "Comparing American History," *Reviews in American History* 10 (December 1982): 64-81; "The Comparative Approach to the Study of Slavery: Problems and

proach is well suited to pursuing the first and to some extent the second of these goals, but it seems to place less emphasis on the third. Needless to say, as a pioneer in considering slavery and antislavery in a broad Western perspective, Davis recognizes the utility of historical comparison. His emphasis in this essay, however, is much more on establishing “international interconnections and influence” than on weighing—and evaluating the significance of—similarities and differences. I would therefore emphasize that even while viewing American slavery as part of a common pattern of Western development, it is also useful to explore the ways in which *differing* historical conditions have shaped variations in this pattern—in this instance, probing the ways in which American slavery differed from slavery elsewhere.³

Second, as a supporter of Davis’s goal of getting away from a parochial approach to American slavery and painting “the Big Picture,” I wonder if it would not be desirable to take an even broader view and to envision an even bigger picture. Although thinking in global terms and seeking “broader insight into world history,” Davis presents his Big Picture primarily as one that integrates American slavery, for too long seen as “a chapter in the history of the U.S. South,” into the general stream of New World slavery. His focus is on what he terms the “*Atlantic Slave System* as well as the place of such *racial* slavery in the evolution of the Western and modern worlds” (*italics added*). But just as it is extremely useful to put American slavery in the context of Atlantic slave systems, so, too, it is desirable, I think, to put those slave systems in the context of unfree labor in general, and its role in the evolution of the modern world. Viewed in this still broader context, American slavery—and New World slavery in general—is a part of what can be termed the “labor question”: who should work for whom, under what terms should work be performed, and how should it be compelled or rewarded? Despite the abolition of slavery in most of the world, the labor question has remained at the top of the agenda for those who would make sense of the way societies are organized.⁴

Examining American and New World slavery in such a broadened context is especially useful when it comes to exploring what Davis refers to as “the cultural

Prospects,” delivered at Conference on Les Dépendances Serviles (Ecole des Hautes Etudes en Sciences Sociales, Paris, June 1996); and “The American South in Comparative Perspective,” delivered at Commonwealth Fund Conference on the Two Souths: Toward an Agenda for Comparative Study of the American South and the Italian Mezzogiorno (University College London, January 1999).

³ Historians comparing slavery in the American South with that elsewhere have delineated some significant differences. Two important examples include the unusual natural growth of the American slave population and the unusually stubborn defense—both verbal and military—of slavery shown by the largely resident American masters. See, among others, C. Vann Woodward, “Southern Slavery in the World of Thomas Malthus,” in Woodward, *American Counterpoint: Slavery and Racism in the North-South Dialogue* (Boston, 1971), 78–106; Richard S. Dunn, “A Tale of Two Plantations: Slave Life at Mesopotamia in Jamaica and Mount Airy in Virginia, 1799 to 1828,” *William and Mary Quarterly*, 3d ser., 34 (January 1977): 40–64; Eugene D. Genovese, *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969), *passim*; Kees Gispens, ed., *What Made the South Different?* (Jackson, Miss., 1990); Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987), 157–91; and Kolchin, *American Slavery, 1619–1877* (New York, 1993), *passim*, esp. 37–40, 189–99.

⁴ For two recent collections of essays that explore this question from a largely New World perspective, see Frank McGlynn and Seymour Drescher, eds., *The Meaning of Freedom: Economics, Politics, and Culture after Slavery* (Pittsburgh, 1992); and Stanley L. Engerman, ed., *Terms of Labor: Slavery, Serfdom, and Free Labor* (Stanford, Calif., 1999).

construction of race.” Although much can be learned about this question by looking at the Atlantic world, a broadened focus is useful here for the simple reason that, whatever their differences, New World slave societies were all based on the subordination of Africans and their descendants to Europeans and their descendants. It is therefore useful to remember (as Orlando Patterson has noted) that slavery did not always involve ethnic distinctions between master and slave. As I have argued elsewhere, comparing the slave-like system of Russian serfdom, which was not based on ethnic or religious distinction, with American slavery underscores the extent to which attributes that have been viewed as “racial” were in fact socially conditioned. The construction of “race” is most clearly evident when we see serfholders who were ethnically indistinguishable from their serfs claiming that those serfs were inherently incapable of freedom—in other words, essentially inventing a racial distinction. In short, a still broader context underscores an important point that is easily lost sight of when all (or almost all) masters are light and slaves dark: exploitation may be facilitated by, but is in no sense dependent on, an ethnic or racial contrast.⁵

In fact, Davis repeatedly approaches the kind of broader contextualization that I am suggesting, without fully embracing it. He describes his lecture course as beginning with the study of slavery in the Bible, which was only by the most generous of definitions a product of the Atlantic world. And near the end of his article, he raises the question of “the great overarching issues regarding slavery, capitalism, and modernity”; pointing to the existence of “virtual slaves” in Asia, Africa, and Latin America, he asks, “what effect, if any, have the great nineteenth-century slave emancipations had on twentieth-century forms of unfree labor?” Davis’s question about the relationship between nineteenth-century slavery and more recent—slave-like—forms of human exploitation clearly calls for more than an Atlantic perspective. It also calls, I think, for more explicit examination of the status of these exploited workers, and of precisely how and when they can be considered to be “virtual slaves.” In short, Davis is raising here the complicated question of the meaning—or definition—of slavery, a question far less self-evident than it at first appears.⁶ Clearly, not all poorly paid, poorly treated workers should

⁵ Kolchin, *Unfree Labor*, 171–73. Examining fifty-seven slaveholding societies across time and space, Orlando Patterson found that “in 75.4 percent slaves and masters were of different ethnic or tribal groups; in 15.8 percent the two were of the same ethnic group; and in 8.8 percent some slaves were from the same group as their masters, while others were from other ethnic groups”; Patterson, *Slavery and Social Death: A Comparative Study* (Cambridge, Mass., 1982), 179. For an influential essay putting forth the now widely accepted notion that race is a social construction (or, in her terminology, an “ideological construct”) rather than a “biological fact,” see Barbara J. Fields, “Ideology and Race in American History,” in J. Morgan Kousser and James M. McPherson, eds., *Region, Race, and Reconstruction: Essays in Honor of C. Vann Woodward* (New York, 1982), 143–77 (quotations 149, 150). Recently, Ira Berlin has argued that the term “social construction” is “not quite right” because race is, more precisely, “a particular kind of social construction—a historical construction”; Berlin, *Many Thousands Gone: The First Two Centuries of Slavery in America* (Cambridge, Mass., 1998), 1. The essential distinction, however, is between the view of race as a fixed biological entity, on the one hand, and as a conceptual category—whether termed “cultural,” “social,” “ideological,” or “historical”—created by human beings, on the other.

⁶ Scholars have differed sharply over this seemingly simple question. Orlando Patterson, for example, has challenged the prevailing view (often implied rather than spelled out) that being owned is “one of the constitutive elements” of slave status; describing slaves as quintessentially outsiders, he defined slavery as “the permanent, violent domination of natally alienated and generally dishonored

be regarded as virtual slaves (although at various times, both defenders of slavery and defenders of labor have found the notion of "wage slavery" to be useful).⁷ Davis's assertion that "the twentieth century has clearly witnessed more slavery than all the preceding centuries combined" would seem to depend on a much more liberal definition of slavery than that embraced by most historians, a definition in some ways reminiscent of metaphorical use of the term by eighteenth and nineteenth-century opponents of "tyranny," including resistance by patriots during the American Revolution to "enslavement" by the British.⁸

Finally, I am not entirely convinced that there have been few contemporary counterparts to the "agitators and petition-signers" who struggled against slavery in the late eighteenth and nineteenth centuries. Of course, current activism does not take exactly the same form that it did in the nineteenth century, but it does seem to me that one can point to examples of the kind of action Davis would like to see. One would be the boycott of trade with South Africa and the associated "divestment" campaign, which played a significant role in bringing about the end of the apartheid regime; another would be the widespread protests against the Nike corporation for engaging in exploitative labor practices in Southeast Asia, protests that the cartoon strip "Doonesbury" has brought to the consciousness of millions of

persons"; Patterson, *Slavery and Social Death*, 17, 13. Such an approach to defining slavery is most common among those who see it as preeminently a system of marginality rather than of labor exploitation; see, for example, Igor Kopytoff and Suzanne Miers, "African 'Slavery' as an Institution of Marginality," in Miers and Kopytoff, eds., *Slavery in Africa: Historical and Anthropological Perspectives* (Madison, Wis., 1977), 3–81. But for assertion of ownership of human property as central to slavery, see, for example, Paul E. Lovejoy, *Transformations in Slavery: A History of Slavery in Africa* (Cambridge, 1983), 1; and Richard Hellie, *Slavery in Russia, 1450–1725* (Chicago, 1982), 29. Noting that "most dictionaries define slaves as property, but most contemporary scholars lean toward natal alienation," Martin Klein recently proclaimed, "I think that both are correct"; Klein, *Slavery and Colonial Rule in French West Africa* (Cambridge, 1998), 15. For an essay cautioning against attempting to distill a universal meaning of slavery divorced from concrete historical circumstances, see Peter Kolchin, "Some Recent Works on Slavery Outside the United States: An American Perspective," *Comparative Studies in Society and History* 28 (October 1986), esp. 768–73.

⁷ See Marcus Cunliffe, *Chattel Slavery and Wage Slavery: The Anglo-American Context, 1830–1888* (Athens, Ga., 1979); David R. Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991), esp. 65–92; Roediger, "Race, Labor, and Gender in the Language of Antebellum Social Protest," in Engerman, *Terms of Labor*, 168–87. Defenders of antebellum slavery routinely insisted that Northern workers were less free than the South's so-called slaves. No one took this argument further than Henry Hughes, who renamed Southern slavery "warranteeism" and insisted that the North's exploited workers were in fact the *real* slaves, but numerous proslavery ideologues resorted to similar word games; see Drew Gilpin Faust, ed., *The Ideology of Slavery: Proslavery Thought in the Antebellum South, 1830–1860* (Baton Rouge, La., 1981), especially essays by William Harper (78–135), James Henry Hammond (168–205), Henry Hughes (239–71), and George Fitzhugh (272–79). See also Kenneth S. Greenberg, "The Proslavery Argument as an Antislavery Argument," in Greenberg, *Masters and Statesmen: The Political Culture of American Slavery* (Baltimore, 1985), 85–103.

⁸ "Virtually every form of oppression has at one time or another been described as a form of slavery," noted Eric Foner in *The Story of American Freedom* (New York, 1998): "In the [Revolutionary] era's political discourse, slavery was primarily a political category, shorthand for the denial of one's personal and political rights by arbitrary government" (29). For a new study that argues that metaphorical "use of slavery as a propaganda vehicle encouraged, and even legitimized, white American prejudices toward black Americans," see Patricia Bradley, *Slavery, Propaganda, and the American Revolution* (Jackson, Miss., 1998), xiv. See also Bernard Bailyn, *The Ideological Origins of the American Revolution* (Cambridge, Mass., 1967), esp. 55–143, 232–46; and Jack P. Greene, "'Slavery or Independence': Some Reflections on the Relationship among Liberty, Black Bondage, and Equality in Revolutionary South Carolina," *South Carolina Historical Magazine* 80 (July 1979): 193–214.

normally apolitical Americans and that may have been partially responsible for the plunge in profits that Nike experienced in 1998.⁹

In concluding, I would like to say how grateful I am to David Brion Davis, both for the kind of work he is doing in developing this course and for sharing his expertise with us. As the study of American slavery experiences a renaissance reminiscent of the extraordinary flowering that occurred in the 1970s,¹⁰ it becomes more and more important for us to put what we know in context, to provide a proper perspective for making sense of the past. With David Brion Davis, this project is in good hands.

⁹ Mistreatment of foreign workers by Nike “has become a popular cause with people all over the country,” reported the *New York Times* (May 18, 1998): A18. “Grade school children write to Nike about it and the company is regularly pilloried in the comic strip ‘Doonesbury.’” Reporting on a demonstration in Denver “as part of International Nike Mobilization to protest reported child exploitation and worker intimidation at Nike’s Asian subcontract factories,” the *Denver Rocky Mountain News* noted that “similar marches were held in all 50 states and several countries Saturday” (April 19, 1998): section Local, ed. F, p. 9A. On the decrease in Nike’s profits, see the *New York Times* (September 22, 1998): C22, and (December 18, 1998): C19.

¹⁰ On the revisionist slavery scholarship that reached a peak in the 1970s, see Peter Kolchin, “American Historians and Antebellum Southern Slavery, 1959–1984,” in William J. Cooper, Jr., Michael F. Holt, and John McCardell, eds., *A Master’s Due: Essays in Honor of David Herbert Donald* (Baton Rouge, La., 1985), 87–111.

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AHR Forum
Small-Scale Dynamics of Large-Scale Processes

REBECCA J. SCOTT

IN HIS PROVOCATIVE ESSAY, David Brion Davis, to whom all of us who work on the history of slavery owe so much, has offered a glimpse of several pedagogical and conceptual strategies that can help overcome the tendency toward the compartmentalization of the study of slavery. He notes two axes along which slavery studies can expand: first, the rich and by now familiar field of *comparative* studies; and, second, what he sees as the less developed area of broadly *systemic* approaches. Some of the latter might be characterized by the term Atlantic Studies. Though rarely invoked as a recognized subfield for the purposes of job definition, Atlantic Studies has indeed emerged as an organizing principle under which multiple phenomena within and outside metropolises and colonies can be linked, often with slavery at the core.¹

Davis's argument for the centrality of slavery, however, goes beyond the importance of the slave trade and slave labor in the construction of the Atlantic system, and invokes the powerful apparent contradiction between the New World as a place of opportunity and new beginnings, on the one hand, and a place of retrograde and exploitative labor relations embodied in slavery, on the other. It is from this vantage point that he asserts that "the Big Picture" is indispensable.

Davis sketches a fascinating undergraduate course focused on the making and then the overthrow of New World slavery, encompassing themes as varied as biblical constructions of bondage and the life histories of the Italian bankers who

I would like to thank Frederick Cooper, Laurent Dubois, David Hancock, Thomas C. Holt, Winthrop Jordan, Lawrence Levine, Aims McGuinness, Peter Railton, Julie Saville, Julius Scott, and Harold Woodman for ongoing discussions of these questions of evidence and interpretation. David Brion Davis has himself been enormously gracious in this continuing debate. My greatest debt is to the residents of Cienfuegos, San Antón, and Pepito Tey, in central Cuba, who have given generously of their time to share recollections of their parents and other kin in the context of the project "Rescate de la Memoria Viva," under the auspices of Orlando García Martínez and the Archivo Provincial de Cienfuegos. Particular thanks are due to Araceli Quesada y Quesada, Caridad Quesada, Ramona Quesada de Castillo, Evelio Castillo, Gerardo Quesada, Francisco Quesada, Humberto Quesada, Tomás Pérez y Pérez, Olga Pérez Ponvert, and Leonardo Alomá.

¹ David Hancock's recent study, for example, places the slave-trading entrepôt of Bance Island, off the coast of Sierra Leone, at the center of his analysis of one set of Anglo-American trade networks. See Hancock, *Citizens of the World: London Merchants and the Integration of the British Atlantic Community, 1735–1785* (Cambridge, 1995). Thomas C. Holt situates both slavery and metropole-colony relations at the heart of his interpretation of liberalism in *The Problem of Freedom: Race, Labor, and Politics in Jamaica and Britain, 1832–1938* (Baltimore, 1992). The list could go on, encompassing works by both historians and anthropologists who have chosen to place colony and metropolis into the same analytic field.

helped to fund slavery in the Mediterranean and Atlantic sugar colonies. The examples he cites provide persuasive evidence that a focus on the *system* of slavery can yield, as he phrases it, “a different way of perceiving world history.”

Nonetheless, I find myself slightly uneasy with the construction of the history of slavery as a systemic Big Picture that will, in Davis’s phrase, serve as “a first step toward coming to terms with the nature and workings of historical evil.” Slavery clearly was a historical evil. The Big Picture is essential. Why, then, does this kind of systems approach strike me as potentially problematic?

I think there might be two reasons. The first is that placing the evilness of slavery at the center, echoing an eighteenth-century view of the slave trade as the “original sin” that lay at the heart of the building of the United States, may lead toward a disproportionate focus on the sinners—in all their multiplicity—and a relative neglect of the sinned against. There is a certain irony here. What in the end makes slavery truly evil is its consequences for those who are subjected to it, yet focusing on the *concept* of evil tends to draw attention back to the evil-doer. If one is going to define a field around the evaluative concepts of sin and evil, I think, one has to complete the explanatory picture by looking as well at the behavior of those who were, in effect, not the sinners.

Second, the systems approach, for all its interpretive potential, tends to draw attention to the history of the actors with a system-wide scope of operation, toward bankers and traders and planters. These figures clearly do constitute a crucial part of the story. But one of the most durable insights of the past decades of scholarship on slavery has been the realization that one must look to the often very local interplay of the actions of slaves, free people of color, masters, nonslaveholding farmers, and the state. The term “agency,” so often invoked to label all this activity, has become a bit shopworn, and holds its own methodological pitfalls in encouraging the scholar to emphasize fragments of autonomy even in situations of what we know to have been extreme constraint. Nevertheless, the search for this kind of evidence has transformed our understanding of slavery and counteracted earlier presumptions of total subordination. Indeed, David Brion Davis has been one of the most important scholarly voices to insist on the recognition of the reciprocities—asymmetrical though they always were—that lay at the heart of slavery. The problem of incorporating an understanding of slave agency into a system-wide analysis, however, remains to a large extent unresolved. An undergraduate course might do well to take advantage of the excitement of the unresolved character of this question within recent scholarship, and grapple with the problem.

In the most dramatic cases, of course, slave agency thrusts itself forward. Serious scholars of the late eighteenth-century Atlantic system now recognize the place of the Haitian Revolution—and thus the agency of the slaves of Saint Domingue—at or near the center of the story. Moreover, the work of Julius Scott and others has demonstrated the existence of complex linkages between slaves and free people, as well as among sailors and stevedores, who managed to send word of the events in Saint Domingue ricocheting from Port au Prince to Havana to Maracaibo to Charleston.²

² See Julius Scott, “A Common Wind: Currents of Afro-American Communication in the Age of the Haitian Revolution” (PhD dissertation, Duke University, 1986).

The greatest difficulty arises as we try to incorporate agency that operates less epochally, that does not yield full-scale rebellion or create a new nation. Working with students to come to terms with this kind of evidence, I think, could be one of the most enlivening challenges in teaching a course of the kind Davis describes. The point is not simply the familiar one of incorporating specialized research findings into a lecture course, or that of finding the voice of subordinated actors. It is a larger one of enriching our notion of historical causation.

We might take as an example the classic theme of the development of the powerful and volatile concept of citizenship in the process and aftermath of the French Revolution. Laurent Dubois, in his recent book, *Les esclaves de la République* (1998), provides us with an account of an intriguing incident on the road between the Guadeloupan capital of Basse-Terre and the village of Trois-Rivières, in April of 1793. The government of the island was in the hands of citizens who identified with the French Republic and feared a royalist coup. On April 21, however, yet more alarming news reached them: hundreds of slaves on the sugar estates around the village of Trois-Rivières had rebelled, killing twenty-two whites. The white citizens and soldiers of Basse-Terre quickly armed themselves and headed in the direction of Trois-Rivières. On the way, they encountered a group of armed slaves, headed toward the city. The soldiers advanced on the slaves, and one challenged them: "Who goes there?" The soldiers were astonished by the reply that came back: "Citizens and friends!"

The slaves came forward to give their version of events. They had discovered that their masters were engaged in a royalist plot to overthrow republican rule on the island, and in order to halt the plot they had found themselves obliged to rebel and kill their masters. The puzzled militia took the slaves into custody and returned to Basse-Terre, the insurgents periodically shouting "Vive la République" as they marched. One of the most intriguing aspects of this event is that the anxious authorities, rather than bringing charges against the rebels for slave insurrection, instead opened an inquiry into the royalist plot in which the planters had apparently been implicated.³

The brief insurrection in Trois-Rivières never became a revolution comparable to that of Saint Domingue, and it nearly vanished from the written record of emancipation in the Antilles.⁴ Retrieving and further analyzing it, as Dubois has done, reveals the extent to which slaves were taking on the active responsibilities of citizenship in a republic and broadly redefining its scope in the process. They claimed for themselves a role their republican rulers had by no means intended they should take. This dynamic, I think, needs to be made central to our understanding of the Big Picture, and it is quite distinct from the issue of the perpetuation of evil under French colonial rule.

Fine, one might say, but what of the non-epochal cases where there is no

³ See Laurent Dubois, "A Colony of Citizens: Revolution and Slave Emancipation in the French Caribbean, 1789–1802" (PhD dissertation, University of Michigan, 1998); and Dubois, *Les esclaves de la République: L'histoire oubliée de la première émancipation, 1789–1794*, Jean-François Chaix, trans. (Paris, 1998), 15–24.

⁴ An initial reference to it does appear in Anne Pérotin-Dumon, *Être patriote sous les tropiques* (Basse-Terre, 1985), 179–82.



Tomás Pérez y Pérez, b. 1902. Photograph by Paul Eiss.

insurrectionary activity at all? How can one show that the behavior of slaves was *consequential* not only for themselves but for the system in which they were enmeshed? Let me try an additional story, this one told to me by Tomás Pérez y Pérez, a ninety-six-year-old former sugar worker who labored nearly all his life in the sugar mill called Soledad, in the old district of Cumanayagua, on the south coast of Cuba.

Tomás Pérez's mother, Bárbara Pérez, was born into slavery on the Pérez Galdós plantation, where she served first as a field laborer and then as a personal servant to the owner's niece. Among her tasks was that of receiving the mail from the mail carrier when his cart came to the door. One day when she brought the mail in, she found the front room of the house empty, so she took the liberty of opening up a newspaper that had come with the mail, to see what she could make of it. Suddenly, the young mistress walked in. Bárbara Pérez quickly folded the newspaper, bent her head, begged pardon, and braced for punishment. The niece, however, told her that there was no need to beg pardon. "Don't tell anyone I saw you, and I'll teach you to read." Bárbara Pérez was evidently an eager student, and by the time abolition came in 1886 she could read and write.⁵

After emancipation, Bárbara Pérez was expelled from the Pérez Galdós plantation. She moved to the nearby town of Arimao, where she worked as a laundress. As her son remembers the story, each time a newspaper arrived in Arimao, Bárbara

⁵ Interviews with Tomás Pérez y Pérez, Cienfuegos, Cuba, March and June 1998.



Bárbara Pérez, b. ca. 1867. Historic photograph, courtesy of the Perez family.

Pérez would read it to the whole town. But how, I asked him, could she read it to the whole town? He explained (in the patient way that you explain something to a person who seems not to understand the obvious) that each neighbor would take a chair from his or her house, and place it on the sidewalk, and sit down, and listen while Bárbara Pérez read.⁶

As a historian, I know of no more vivid image of the construction of the public

⁶ Interviews with Tomás Pérez, 1998. Bárbara Pérez was also an avid fan of novels, and she apparently read vigorously until the end of her very long life. For a discussion of Tomás and Bárbara Pérez in the context of central Cuba at the turn of the century, see Rebecca J. Scott, "Race, Labor, and Citizenship in Cuba: A View from the Sugar District of Cienfuegos, 1886–1909," *Hispanic American Historical Review* 78 (November 1998).

sphere than this picture of a woman recently enslaved, now supporting herself by washing clothes, who with her neighbors creates a public ritual and a space for the discussion of news from the wide world in the dusty, riverfront town of Arimao. One could carry the story forward, to the outbreak of the Cuban War of Independence in 1895, tracing the capacity of the well-informed residents of Arimao to mobilize in support of the Liberation Army. But even if we freeze the frame on the image of the recently emancipated Bárbara Pérez, *lectora* (reader) to the town of Arimao, we have captured a crucial element in the postemancipation construction of citizenship.

Finding and recounting stories like that of the insurrection at Trois-Rivières or of the public readings at Arimao will not, by itself, solve the vexed question of how to integrate micro-level data into macro-level explanations. But we might begin by assuming that a course aimed at conveying a *systemic* understanding of slavery will work best if it also conveys multiple *dynamic* explanations. To the extent that such dynamics can be convincingly located in the agency of subordinate actors, it should be possible to tell these stories as part of even the most ambitious Big Picture, not merely as illustrative sidebars.

The encounter of French republicanism with slavery was both a set of epoch-making intellectual developments in Paris and a meeting on the road to Trois-Rivières after which things in Guadeloupe could not be the same again. The redefinition of citizenship for former slaves in Cuba was both a complex defensive stratagem on the part of the Spanish colonial state and the seizing of the skill of reading by Bárbara Pérez, laundress and later midwife, who was determined that the community around her was entitled to aspire to more than mere survival.

We might close with a final image, also from the sugar country of south-central Cuba. Cayetano Quesada was born in 1879 to slave parents on the Santa Rosalía plantation and came of age during the transition from slavery to free labor. In October of 1895, he joined a small band of local rebel forces seeking to link up with the insurgency that had emerged to the east, which fought both against Spanish colonial rule and against the racial hierarchy built by a society based on slavery. Cayetano Quesada served as a private in the rebel Cienfuegos Brigade until near the end of the war in 1898. After the war, he and his fellow veteran Ciriaco Quesada settled on adjacent plots of land in the village of San Antón, at the edges of the Santa Rosalía and Soledad plantations. They raised cattle and horses, and planted corn, peanuts, sweet potatoes, and plantains.⁷

One hundred years later, in June of 1998, Cayetano Quesada's daughter Ramona Quesada, who lives with her husband, Evelio Castillo, on that same plot of land, was understandably surprised to find several historians walking up the pathway to her door, archival notes in hand. The ensuing conversation, as we sat together with her family on the patio, was an exchange of memories, notes, and reflections on the past. Recalling the stories she had heard from her father and others, she gave a harrowing account of the death of one of Cayetano Quesada's brothers during

⁷ For a detailed discussion of the experiences of Cayetano and Ciriaco Quesada, see Rebecca Scott, "Reclamando la mula de Gregoria Quesada: El significado de la libertad en los valles del Arimao y del Caunao, Cuba, 1880–1899," *Islas e imperios* (Barcelona) 2 (April 1999).



Ramona Quesada de Castillo, b. 1935. Photograph by Jack Kenny, 1998.

slavery and spoke of the struggle of the former slaves of Santa Rosalía to obtain and hold on to land. As evening fell, Ramona Quesada reflected on our encounter, both in terms of family and in terms of history. First, she expressed her pleasure at talking about her father and learning that documents about him had survived: “It is a joy to know this.” Then she made an observation that speaks to the pedagogical and interpretive questions at hand. “I thought that my father’s effort . . . had been erased from the history of Cuba.”⁸

The word Ramona Quesada chose—effort (*esfuerzo*)—seems particularly appropriate. It was not just that Cayetano Quesada’s name and experiences should be remembered. Rather, it was that history itself should recall his *effort*—an effort aimed, among other things, at altering the large structures of discrimination, impoverishment, and colonialism in Cuba. Ramona Quesada was just ten years old when her father died, and she seems to have tried over her lifetime to find the place that her memories of him might occupy within a more abstract narrative of Cuba’s national history. We, too, must try—stimulated by David Brion Davis’s proposal

⁸ We were a group of four visitors: Orlando García Martínez from Cienfuegos, Leonardo Alomá from Pepito Tey (the former Soledad), and Aims McGuinness and myself from the University of Michigan. The June 1998 interview with Ramona Quesada and Evelio Castillo was ably transcribed by Evelyn Baltodano.

and by Ramona Quesada's memories—to envision a way of teaching the history of slavery that can portray the breadth and complexity of the slave system into which Cayetano Quesada was born, and of the society that he helped to build after its destruction.

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AHR Forum
Slavery at Different Times and Places

STANLEY L. ENGERMAN

THE TITLE OF THE SESSION at which these papers were originally presented, "Looking at Slavery from Broader Perspectives," can refer to at least three different issues. First, slavery has been one of the most ubiquitous of human institutions, and has existed in many places. It has been present in societies dominated by all major religions and ideologies, and had legally lasted in some places into the second half of the twentieth century—if not more informally in places until the present day. Although there are important differences in its economic and legal basis, certain characteristics regarding who could be enslaved or who could be bought and sold had important similarities, and the study of these differences and similarities provides a useful basis for numerous comparative studies related to the understanding of human behavior and social institutions. Second, slavery, when it existed, should not be examined in isolation from other institutions and happenings at that or other times. Thus it is important to trace the various linkages of slavery with the nonslave aspects of different societies. Third, related to the second, the previously sharp line between slavery as *the* evil and other labor and social systems that are therefore seen as quite different, and thus somehow more acceptable, has now become blurred, pointing to the usefulness of more detailed comparisons of the legalities and actualities of various types of social and labor institutions.¹

One basic problem that must be dealt with is the precise definition of slavery and, correspondingly, of nonslavery. Definitions are generally a rather dull topic, but, in the case of slavery, they have been central to the understanding of nonslavery or freedom. Any specific definition of slavery has legal, cultural, political, and economic aspects, and it is often hard to know exactly where to draw the line among labor institutions as well as between legal slavery and the use of slavery as a metaphor for any form of human poverty and domination. If slavery is regarded as a unique mode of control of individuals, this would seem to make all nonslavery appear as freedom and, therefore, to be regarded as a progressive and desirable development. If however, slavery is regarded as only one part, or one end, of a spectrum of controls, then some would argue that this makes slavery seem less evil and more benign than it was, compared to other forms of social control.

The many past and present uses of the analogy of slavery in describing social and labor systems suggests that few, if any, of slavery's controls are by themselves

¹ See the essays in *Terms of Labor: Slavery, Serfdom and Free Labor*, Stanley L. Engerman, ed. (Stanford, Calif., 1999).

unique. Nevertheless, the use of the term slavery as a descriptive noun means, as David Brion Davis suggests, that slavery is usually considered to be the greater evil, although it remains unclear by how much. Nor, as Davis also suggests, is it clear that individual ownership must have always been necessarily worse for the people than absolute control of the entire population by a state's political apparatus.

Part of the problem of defining and comparing slavery and freedom is indicated by Davis's description, based on the writings of Ruth Karras, of the ending of medieval slavery.² Slavery ended on economic grounds, in this argument, when there was no longer a surplus produced by slaves that made it profitable for owners. The ending of the need to use slavery to get people to labor occurred when all people, free as well as enslaved, produced only at a subsistence level of income. In such circumstances, freedom and liberty are not conditions of any great economic or political advantage to the laborer or to the landowner, since either case implies coercion of all labor, the mechanism now being hunger and starvation, not force. In this sense, slavery would end when the need to labor to achieve subsistence by the free population occurs because of the natural constraints related to hunger. This is one example of restricted gains to be expected with freedom from slavery. Davis notes other cases, including those in which ending slavery implies an acceptance of Spiritual Bondage, which some, of course, might still consider freedom if it was voluntarily accepted. In many societies, freedom via manumission still implied obligations to former masters. Thus the end of legal enslavement, to an individual or society, need not mean unrestricted freedom in the most meaningful sense of the word.³ The relation of emancipation to the disappearance of the economic surplus has long been argued. To Moses Finley, the decline in ancient slavery was due to the disappearance of the surplus, while George Tucker and Abraham Lincoln, among others, used a similar argument to predict the date of the probable demise of Southern slavery in the United States in the antebellum period.⁴ Indeed, the influential writings of the Dutch ethnographer H. J. Nieboer argued that the existence and the ending of slavery would be based on the possible surplus of production above subsistence, with higher surpluses making slavery more probable.⁵

There is a related problem in defining and understanding slavery that is seldom discussed, since it obviously does not apply to New World slavery, which was clearly involuntary in its origin and continuation. Elsewhere, however, there were frequent examples of what can be described as voluntary slavery, of individuals and/or their family members being sold into slavery, in order to avoid starvation, murder, infanticide, abandonment, human sacrifice, or related circumstances. Similarly, in some explanations of the rise of serfdom, emphasis is given to the desire or requirement of the serf to obtain political and economic protection from more

² Ruth Mazo Karras, *Slavery and Society in Medieval Scandinavia* (New Haven, Conn., 1988).

³ Orlando Patterson, *Slavery and Social Death: A Comparative Study* (Cambridge, Mass., 1982), 240–61.

⁴ M. I. Finley, *Ancient Slavery and Modern Ideology* (London, 1980), 123–49; George Tucker, *Progress of the United States in Population and Wealth in Fifty Years* (1855; rpt. edn., New York, 1964), 108–18; and Roy Basler, ed., *The Collected Papers of Abraham Lincoln*, vol. 3 (New Brunswick, N.J., 1953), 181.

⁵ H. J. Nieboer, *Slavery as an Industrial System: Ethnological Researches* (The Hague, 1900), 169, 296, 425–26.

powerful authorities. How is the choice to become a slave or a serf to be evaluated, since in Western philosophy it has long been argued that the right to enslave (and also to kill) oneself was prohibited? Are there any conditions under which we would regard such enslavement as acceptable? For example, while voluntarily enslavement tended to be more frequent in Asia and Africa, it was no longer formally approved in early modern Europe, where a similar set of conditions led to infanticide and child abandonment, not enslavement. Shouldn't we try to better understand the conditions that made, at that time, slavery seem the lesser evil and voluntary enslavement a desired choice for individuals to make?

This question of voluntary slavery has certain contemporary implications, given Davis's discussion at the end of his article on the present-day role of multinational corporations as users of what are called virtual slaves. Certainly, working conditions are difficult, but the willingness of members of these populations to accept them suggests the belief that, given current political and economic realities, the consequences of what we consider to be a more moral set of actions may not be regarded as an unmixed blessing. As in the case of slavery in the Americas, we tend to regard those buying slaves as committing a greater evil than those selling, in paying for and using the enslaved to produce in order to make profits. Less is often said about the role of the sellers of slaves, but without them, obviously, the slave trade and slavery would not have existed. Why were some people willing to enslave and to sell other people? This may be as puzzling to understand as why some people were willing to sell themselves into slavery. Until we can better understand the nature of the alternative available at any time, however, we may find it difficult to appreciate the social and economic problems that freedom creates.

We have recently learned more about the nature of the origins of the slave trade within Africa, with the role of African agency as enslavers and buyers and sellers of people, while others in Africa were being enslaved and sold. There was, in Africa and elsewhere, a relation between slavery and the development of states and of increasing wealth. The basis of such a willingness to sell people (as also had taken place among Europeans, who were earlier willing to enslave other Europeans and later still willing to kill them), whether for economic, political, or religious reasons, did vary over time and disappear in some areas, but the long persistence of a willingness to enslave, and its importance to many societies, is as clear as it remains difficult to explain, even on what some might regard as purely economic grounds.⁶

Debates on the link of racism and slavery have a long history, and certainly the relationship seems clear to many in the modern world. Yet it is useful to remember that the specific outcome of racism is not always obvious, and detailed examination of particular historical events is important. At times, racism is necessary for slavery, providing the basis for the enslaveable outsider. Yet, as William Evans has indicated, who can be acceptably considered to be an outsider has varied historically.⁷ The insider and outsider distinction itself may imply a rather wide variety of different social outcomes. Europeans, after about the start of the thirteenth

⁶ David Eltis, "Europeans and the Rise and Fall of African Slavery in the Americas: An Interpretation," *AHR* 98 (December 1993): 1399–1423.

⁷ William McKee Evans, "From the Land of Canaan to the Land of Guinea: The Strange Odyssey of the Sons of Ham," *AHR* 85 (February 1980): 15–43.

century, began to regard other Europeans, whom they were still willing to kill, burn, torture, and pillage, as now being unenslaveable.

Racism, moreover, did not always lead to a desire to enslave and to increase the numbers enslaved. Some variants of racism led to antislavery arguments, or at the least to the argument to keep slaves out, either to encourage settlement by nonslaves or else to permit a more homogeneous society for the present population. In the Americas, the argument to restrict slavery by limiting immigrants from Africa is found rather early and continued through the ending of the slave trade, forming an important part of the antislave trade and antislavery crusades. In the United States, the Northern antislavery movement also included an anti-black component, as it did elsewhere in the Americas.

The complexities of racism can be illustrated by the Australian experiences with a particular form of coerced labor, indentured labor from the Pacific Islands, used to produce sugar in Queensland between 1870 and 1900.⁸ Racism at first led to the purchase (or, some claim, kidnapping) and importing of contract labor; then, when a generation or so later a white Australia was desired, racism meant keeping the islanders out, forcing the return of those already there, and paying subsidies to white producers of sugar. Here, clearly, racial attitudes persisted, but without leading to an unvarying set of actions over time regarding labor institutions. And in these debates, there were even some estimates introduced of the probable costs to white Australians of the second racist policy, exclusion, because of the higher price of sugar necessarily paid to support production by whites.

There is a central paradox pointed to by Davis, one that had been earlier raised by Nieboer. Nieboer argued that very poor societies would not have slavery, since they could not generate the surplus production that would make ownership of human chattels worthwhile. Slavery would therefore exist only in societies in which levels of income rose above subsistence, meaning that productive progress would lead to the rise of slavery. Davis notes that the European nations most involved in the transatlantic slave trade had high levels of cultural and economic achievement, which might be considered to be a violation of the spirit of the Enlightenment hope that all good things would go together. Unfortunately, this paradox can be found in many other cases, with a rather pointed nonslave example provided by Orson Welles in the movie *The Third Man* (1949).⁹ Justifying his work in the black market in post-World War II Vienna, Welles's character, Harry Lime, comments that while in Italy for thirty years under the Borgias they had warfare, terror, murder, bloodshed, they also produced Michelangelo, Leonardo da Vinci, and the Renaissance. In Switzerland, they had five hundred years of democracy and peace, and what did they produce?—the cuckoo clock. Whatever the accuracy of Welles's account, this paradox, seen in many slave and nonslave societies, is clearly not an infrequent historical development, whether in regard to cultural, political, or economic factors.

I would also like to make explicit another point implicit in Davis's proposals to broaden historical coverage by calling for a more systematic linking of the study of

⁸ See *Report of the Royal Commission on the Sugar Industry*, Government of the Commonwealth of Australia (1912), particularly xx, xxi, 553.

⁹ Frank Brady, *Citizen Welles: A Biography of Orson Welles* (London, 1990), 450–51.

slavery with the study of abolition and with the study of post-emancipation adjustments in ex-slave societies. By learning more about the behavior of ex-slaves and examining their behavior when at least some of the previous legal restrictions were removed, we should also learn much more about the impact of slavery and its complex psychological and social effects. This would entail the examination of the rather different circumstances of life under slavery (urban, small farm, or large plantation, for instance) in conjunction with the post-emancipation responses. We would not expect the latter to be unrelated to the events of the preceding centuries. This need not be simple and direct, since both in the discussion linking the slave family and the black family today and in the examination of the development of twentieth-century racism, detailed studies remind us that the long period of racial segregation and legal discrimination, as well as the prior slave era, had a substantial impact on the emergence of our contemporary conditions.

Looking at post-emancipation adjustments by different societies will also help us begin to work on an answer to Davis's last question. One characteristic of the study of post-emancipation societies is the opinion that such dramatic changes as emancipation had outcomes that were ultimately discouraging and disappointing for just about everyone. The optimistic predictions for a successful future (as with those for many other social and political changes) were seldom realized, and, while things may not have really gotten worse, many problems persisted and things seldom got to be as good as hoped for. It is these failures that generally seem to have attracted attention. Perhaps these so-called lessons, pessimistic as they appear, have influenced subsequent concerns and evaluations of policies to deal with contemporary conditions. What is called apathy may be due as much to the pessimism generated by the perceived negative outcome of earlier changes as to indifference or cynicism about the need for societal changes. The focus on negative aspects of past changes, whether by those who had their fears confirmed or else their fondest hopes left unrealized, may, however, lead to an understatement of what was accomplished. This may have had some unfortunate effects in discussions of policies for the future. Maybe more studies of past changes such as abolition and emancipation, incomplete successes though they often seem, can provide important insights and understanding concerning what can feasibly be done to bring about social improvement, the goal of which is, after all, why we regard the study of past slavery as being of such importance.

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Review Essays
Counting and Power

*The publication of Alfred W. Crosby's **The Measure of Reality: Quantification and Western Society, 1250–1600** raises anew the critical issue of Western power in the modern world. These three reviews tackle that issue through analyses of Crosby's book. They are another installment in our periodic publication of multiple reviews of books that address issues of broad disciplinary concern as a means of sparking debate among historians of various times and places. In this case, Crosby continues his now multi-volume quest to understand the success of European imperialism by presenting a striking argument that quantification was the fundamental source of Western imperial power in the late medieval and early modern eras. Distinctive methods of counting and measurement and the unique mindset that they promoted resulted in scientific, navigational, military, and other innovations that led to Western world dominance. He develops this argument in a wide-ranging and provocative argument that itself spans the globe, though it concentrates on developments in Western Europe. **Roger Hart, Margaret C. Jacob, and Jack A. Goldstone** approach the book and its larger subject from various perspectives. Their reviews combine to create a broad analytical framework for analyzing Crosby's argument and the issue of Western imperialism.*

Review Essays
The Great Explanandum

ROGER HART

ALFRED W. CROSBY'S *The Measure of Reality: Quantification and Western Society, 1250–1600* is a broad survey of measurement animated by captivating examples drawn from four centuries of mathematics, astronomy, music, painting, and bookkeeping. The thesis he develops from this survey is that the unique success of European imperialism can be explained by a shift in Western *mentalité* in the late thirteenth century from a qualitative to a quantitative perception of reality. The larger significance of this book is that it is one of the more careful attempts to provide rigorous argument and historical documentation for a set of themes common in the historical literature of the twentieth century—radical breaks in Western thought and the uniqueness of the West. And as such, the shortcomings of Crosby's book suggest some of the larger problems in an entire genre of world history.

The more focused historical problem that Crosby addresses—setting aside for a moment larger claims about the West and its uniqueness—is an exceedingly complex one: the explication of a series of historical contingencies that led to the superiority of specific European empires (and not others) as of the nineteenth century in weaponry, navigation, and bureaucratic administration. Tracing any facet of these developments in Europe is a monumental task. Weaponry alone, for example, requires analyses of the developments in chemistry, metallurgy, and technology, to name a few, that transformed cannons from ineffective to deadly weapons.¹ Another aspect of the explanation would be the enormous manpower invested in the study of cannons by Italian mathematicians,² and the fortuitous fact that the problems of ballistics turned out to have mathematical solutions. But a complete explanation would also require comparative analysis, examining why other nations invested less in developing these particular technologies. The most obvious approach to explaining superiority in weapons, then, might be a comparative history of weapons.

However, victors of military conquest often prefer loftier explanations for their superiority; and Europeans are hardly unique in their attempts to attribute their success to more noble cultural traits claimed to be uniquely their own. Crosby

¹ For one example, see Bert S. Hall, *Weapons and Warfare in Renaissance Europe: Gunpowder, Technology, and Tactics* (Baltimore, 1997).

² See Mario Biagioli, "The Social Status of Italian Mathematicians: 1450–1600," *History of Science* 17 (1989): 41–95.

rightly rejects at the outset as “hilariously unlikely” the modern variants of these apologetics—ones we are now likely to view as perniciously racist—that Westerners were “the most recent, highest, and, in all likelihood, final twigs on the exfoliating tree of evolution” or that they “were the brightest, most energetic, most sensible, most aesthetically advanced, and most ethical humans.”³ Crosby fails to note, however, that what such claims share with his own approach is a confidence in Western uniqueness: unique civilizations deserve unique explanations, and the usual kinds of explanations we grant for the military successes of Alexander the Great or the Mongols simply will not do.

One of Crosby’s previous works, *Ecological Imperialism: The Biological Expansion of Europe, 900–1900* (1986), offered an important alternative to the usual genres of explanation criticized above. That is, against conventional historical explanations for European victories as the inevitable result of superior technology, and against apologetics that explained European success as resulting from their superior culture (including claims that link culture to science and weapons),⁴ Crosby uncovered unexpected factors that played a crucial role in European conquests—“the biological advantages that the white imperialists enjoyed” including diseases, animals, and plants. *Measure of Reality* is, Crosby notes, his third book in his “lifelong search” to explain “the amazing success of European imperialism.” Crosby here rejects his previous thesis as “biological determinis[m]”; he seeks to explain why “Europeans were incomparably successful at sending ships across oceans to predetermined destinations and at arriving at those destinations with superior weaponry—with, for instance, cannons superior to those of the Ottomans and the Chinese.”⁵

His answer is quantification.⁶ “Westerners’ advantage, I believe, lay at first not in their science and technology, but in their utilization of habits of thought that would *in time* enable them to advance swiftly in science and technology and, in the meantime, gave them decisively important administrative, commercial, navigational, industrial, and military skills. The initial European advantage lay in what French historians have called *mentalité*.” He argues there was a shift from a qualitative *mentalité* he terms the “Venerable Model” to a “New Model” of reality—a “devotion to breaking down things and energies and practices and perceptions into uniform parts and counting them,” which he terms “quantification.”⁷ Crosby pinpoints this crucial shift between 1275 and 1325, when “someone built Europe’s first mechanical clock and cannon, devices that obliged Europeans to think in terms of quantified time and space,” along with “*Portolano* marine charts, perspective painting, and double-entry bookkeeping” from approximately the same period. Crosby has thus located, he contends, the revolution that fundamentally transformed Europe: “There was nothing quite like this half century [1275 to 1325] again until the turn of the twentieth century, when radio, radioactivity, Einstein,

³ Alfred W. Crosby, *The Measure of Reality: Quantification and Western Society, 1250–1600* (Cambridge, 1997), ix–x.

⁴ For a striking analysis of twentieth-century apologetics, see Norton Wise, “Pascual Jordan: Quantum Mechanics, Psychology, National Socialism,” in Monika Renneberg and Mark Walker, eds., *Science, Technology, and National Socialism* (New York, 1994).

⁵ Crosby, *Measure of Reality*, x, ix.

⁶ Crosby does not clarify here whether disease, animals, and plants remain factors in his explanation of European conquests.

⁷ Crosby, *Measure of Reality*, xi, 11, italics in original.

Picasso, and Schönberg swept Europe into a similar revolution.” The result: in comparison with other cultures, Europeans “were thinking of reality in quantitative terms with greater consistency than any other members of their species.”⁸

What historical evidence does Crosby offer for this thesis? The first problem is his claim about quantification in the West: he never really offers rigorous criteria for determining what is to count as quantification. His examples include a very broad sampling from commercialization, calendars, clocks, and maps to perspectival painting and even musical notation.⁹ However plausible his claim of the increasing applications of measurement might at first seem, Crosby’s examples add little to a historical analysis of the specific time and location of these applications, their extent, and their rate of change over time. The examples he presents, then, provide very little basis to justify his claim of the marked “shift” that occurred in the late thirteenth century (rather than in, for example, the eighteenth century), and even less of a basis for any comparison with other civilizations.¹⁰ Quantification—its extent, location, dates, and purported shifts—remains unquantified.

Crosby’s link between quantification in general and the specific technical developments in weapons and navigation is equally weak. In the chapter on mathematics, after briefly mentioning Leonardo Fibonacci, the thirteenth-century Italian mathematician, the majority of the chapter describes the change from Roman numerals to Arabic (along with the asserted decline of number mysticism); he offers little analysis of important developments in mathematics during the period. The same is true of his discussion of music and astronomy. Without technical analyses, it remains impossible to understand the emergence of the techniques in navigation and weaponry that Crosby seeks to explain.¹¹

An even more serious problem is that, although his overall thesis purports to be fundamentally comparative, Crosby presents hardly any analysis of non-Western civilizations. For example, his few scattered remarks on China range from hackneyed stereotypes (the “Chinese had forgotten the giant clocks of the Song Dynasty, and their calendar was defective and stayed that way until the Jesuits helped them fix it”) to statements that are unintelligible (“the theoretical and practical eventually diverged”) or simply false (“unlike the societies of the East the West was hungry to learn by staring at standardized marks on paper”).¹² Crosby offers very little analysis of Chinese astronomy, mathematics, printing, or music, to name a few. Yet secondary scholarship—in English—on these topics is both easily accessible and well known. For example, many of these subjects constitute an entire

⁸ Crosby, *Measure of Reality*, 19, see also 227; xi.

⁹ Interestingly, he argues that “the individuals chiefly responsible for the New Model were townspeople” (58), and “bookkeeping has done more to shape the perceptions of more bright minds than any single innovation in philosophy or science” (221).

¹⁰ For example, John Heilbron argues that a key shift in quantification occurred in 1760. “Introductory Essay,” in Tore Frängsmyr, J. L. Heilbron, and Robin E. Rider, eds., *The Quantifying Spirit in the 18th Century* (Berkeley, Calif., 1990), 2–3. The problem of comparison with other civilizations is discussed below.

¹¹ Crosby’s book is apparently intended for a broad audience, but this does not explain the absence of technical analysis. For a classic example of a book that summarizes technical arguments for a broader audience, see Thomas S. Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (Cambridge, Mass., 1957).

¹² Crosby, *Measure of Reality*, 17, 231.

volume in Joseph Needham's *Science and Civilisation in China*. Crosby does not mention even the two volumes on military technology.¹³ A considerable body of more recent research is also easily available.¹⁴ Instead, in his passing references to China, Crosby chooses to cite purportedly comparative studies that present grand claims about the development of science without ever bothering to examine science.¹⁵ Other civilizations fare even worse than China, meriting only passing mention. Crosby's failure to incorporate easily available secondary sources on non-Western civilizations makes it impossible to take any of his comparative claims seriously.

The problem here for Crosby's thesis is that from these secondary sources one could easily write a similar history of quantification, measurement, and standardization in China, drawing examples from commercialization, census, taxation, land measurement, medicine, mathematics, astronomy, and even music. To offer but one example, in the search to refine court music, one scholar in the late sixteenth century discovered the equal tempered scale and calculated the twelfth roots of two to twenty-five decimal places. The Chinese were quantifying, but doing less technical work on cannons or navigation. Quantification, then, did not itself lead to advances in weapons. The apparent lack of quantification in other civilizations—the centerpiece of Crosby's argument against which the accomplishments of the Europeans chronicled in his book appear so impressive—is just the result of his own failure to cite readily available examples from secondary historical studies.

The lack of historical evidence for Crosby's claims is covered over by his shift from the empirical to a series of claims about thought, science, and their relationship to civilizations; these claims constitute the weakest part of this book. Again, the first problem is the lack of any critical analysis of many of the key terms within which Crosby frames his central thesis—terms such as “reality,” “Old Model,” “New Model,” “science,” and *mentalité*. He parenthetically defines reality as “everything material within time and space, plus those two dimensions per se.” The Old Model “perceived reality as an uneven, heterogeneous sort of thing.” The New Model “was simply this: reduce what you are trying to think about to the minimum required by its definition; visualize it . . . divide it . . . into equal quanta. Then you can measure it, that is, count the quanta.” “Science (and a great deal else

¹³ *Science and Civilisation in China* (Cambridge, 1954–) presently includes the following volumes: Vol. 1, *Introductory Orientations*; vol. 2, *History of Scientific Thought*; vol. 3, *Mathematics and the Sciences of the Heavens and the Earth*; vol. 4, *Physics and Physical Technology*—pt. 1, *Physics*, pt. 2, *Mechanical Engineering*, pt. 3, *Civil Engineering and Nautics*; vol. 5, *Chemistry and Chemical Technology*—pt. 1, *Paper and Printing*, pt. 2, *Spagyric Discovery and Invention: Magisteries of Gold and Immortality*, pt. 3, *Spagyric Discovery and Invention: Historical Survey, from Cinnabar Elixirs to Synthetic Insulin*, pt. 4, *Spagyric Discovery and Invention: Apparatus, Theories, and Gifts*, pt. 5, *Spagyric Discovery and Invention: Physiological Alchemy*, pt. 6, *Military Technology: Missiles and Sieges*, pt. 7, *Military Technology: The Gunpowder Epic*, pt. 9, *Textile Technology, Spinning and Reeling*; vol. 6, *Biology and Biological Technology*—pt. 1, *Botany*, pt. 2, *Agriculture*, pt. 3, *Agro-Industries and Forestry*; vol. 7, pt. 1, *Language and Logic*.

¹⁴ For a general bibliography of works on Chinese history, see John King Fairbank and Merle Goldman, *China: A New History*, enl. edn. (Cambridge, Mass., 1998), 459–514. For an overview of current research in Chinese science, see Nathan Sivin, “Science and Medicine in Imperial China—The State of the Field,” *Journal of Asian Studies* 47, no. 1 (1988): 41–90.

¹⁵ Derk Bodde, *Chinese Thought, Society, and Science: The Intellectual and Social Background of Science and Technology in Pre-Modern China* (Honolulu, 1991); Toby E. Huff, *The Rise of Early Modern Science: Islam, China and the West* (Cambridge, 1993).

characteristic of modern societies) can be defined as the product of the application of mathematics, with its Platonic precision, to Aristotle's crude realities."¹⁶ He offers for these simplistic caricatures neither references nor further explanations, neither historical evidence from the period nor any analysis from modern scholarship.¹⁷ To tie these together, Crosby turns to the notion of *mentalité*; his remark, "assuming eras do have zeitgeists," alerts the reader that there will be no critical discussion of the reasons that this concept has proven so unsatisfactory.¹⁸ This then provides the framework for rhapsodic pronouncements about shifts in the Western perception of reality. To this mix, Crosby adds "visualization." Ultimately, in place of historical explanation, Crosby can do no better than offer the analogy "striking the match" (his title for Part 2 of his book) to explain its relationship to quantification.

Crosby's claim to have found the underlying cause of an asserted great shift in Western thought is, of course, not new. In fact, claims about radical shifts in Western thought were a central theme of the history of science in the first half of the twentieth century; precisely what that shift was and when it occurred was a central topic of debate. For Pierre Duhem, it was debates over Aristotle and the Arab philosopher Averroës in Paris in the fourteenth century; for Alexandre Koyré, it was the shift from the closed world of the ancients to the infinite universe of the moderns; for Thomas Kuhn, it was the change from a geocentric to a heliocentric model in astronomy; for A. C. Crombie, it was the experimental work of the thirteenth-century English natural philosopher Robert Grosseteste; and this is to name only a few of the most important theses from the history of science.¹⁹ Crosby does not contextualize his own claim within this body of literature. More importantly, Crosby does not address the important conclusions of recent research in the history of science—the growing skepticism toward claims for great shifts, and in particular, the emerging consensus that there was no "scientific revolution."²⁰

Lacking rigorous criteria for what quantification is, a way to measure it, a causal connection between quantification and technical innovations in weapons and navigation, and lacking any comparative analysis, Crosby's thesis about Western quantification clearly does not begin with empirical evidence and proceed toward historical explanation. Instead, it begins with the Great Explanandum: What differentiates the West from the rest? What makes the West unique? This thesis of the uniqueness of "the West" (setting aside momentarily the problem of what the West might be) requires some preliminary clarification. For it might seem at first to have a simple enough answer: only in the West does one find Plato, Shakespeare,

¹⁶ Crosby, *Measure of Reality*, 23, 228, 16.

¹⁷ For a useful bibliography focused on the seventeenth and eighteenth centuries but also including work covering earlier periods, see Steven Shapin, *The Scientific Revolution* (Chicago, 1996). For a discussion of issues on science, see Peter Galison and David J. Stump, eds., *The Disunity of Science: Boundaries, Contexts, and Power* (Stanford, Calif., 1996).

¹⁸ Crosby, *Measure of Reality*, 11. See, for example, G. E. R. Lloyd, *Demystifying Mentalities* (Cambridge, 1990).

¹⁹ Pierre Duhem, *The Aim and Structure of Physical Theory* (Princeton, N.J., 1991), trans. of *La théorie physique: Son objet et sa structure* (Paris, 1906); Alexandre Koyré, *From the Closed World to the Infinite Universe* (Baltimore, 1957); Kuhn, *Copernican Revolution* (1957); A. C. Crombie, *Robert Grosseteste and the Origins of Experimental Science, 1100–1700* (Oxford, 1958).

²⁰ For an introduction to this literature, see Shapin, *Scientific Revolution*.

and Galileo; only in the West does one find Euclidean geometry, Aristotelian logic, and Copernican astronomy. Yet in no civilization other than China can one find Confucius, Mozi, and Zhu Zaiyu; only in China can one find the iron and steel production of Kaifeng, celestial-origin algebras, and the voyages of Zheng He. The question, then, cannot be what makes the West unique but rather what makes the West *unique*—if all civilizations are to be in some sense unique, some are more unique than others.

The variant of European uniqueness that Crosby initially sets out to explain is “the amazing success of European imperialism. Europeans were not the cruelest and not the kindest imperialists, not the earliest and not the latest. They were unique in the degree of their success. They may retain that distinction forever.” This success is unique because “Cyrus the Great, Alexander the Great, Genghis Khan, and Huayna Capac were great conquerors, but they were all confined to no more than one continent and at best a wedge of a second. They were homebodies compared with Queen Victoria.”²¹ Here, then, is what makes the West *unique*: instead of a single empire dominating a single continent, several empires (all combined in this accounting under the rubric “the West”) dominated several continents. Elsewhere, Crosby offers other claims about what made the West unique: it chose “to perceive as much of reality as possible visually and all at once”; it was able “to bring mathematics and measurement together and to hold them to the task of making sense of a sensorially perceivable reality”; it was “uniquely prepared to survive and even to profit from such an avalanche of [social] change”; it was “unique in its enthusiasm for clocks”; it was “advancing faster than any other large society in its ability to harness and control its environment.”²² To this incongruous list, we must add, of course, quantification and the New Model of reality. Crosby’s thesis thus only adds to an already extensive literature on Western uniqueness: a very short list includes language (alphabetic scripts, the copula), economics (capitalism, double-entry bookkeeping), religion (Puritan ethic, disenchantment), philosophy (natural law, causal thinking, perception of time and space, demonstrative logic), and politics (democracy). The search for the key features distinguishing the West from the Rest continues to this day.²³ Apparently—contrary to these assertions—there is no unique answer to what makes the West *unique*; given that the West is unique, almost any explanation will do.²⁴

The Great Explanandum begins with a credulity toward “the West” as a fundamental category of historical explanation. Recent work in cultural criticism has questioned nations as “imagined communities”; yet this same literature has too often failed to analyze the ways in which the West is imagined. *The Measure of Reality* itself provides one striking example: the West (which for Crosby begins in the Neolithic period, includes Greece and ultimately Europe) becomes reified and anthropomorphized (it “stumbled down,” “invented,” “fizzed and bucketed”;

²¹ Crosby, *Measure of Reality*, ix.

²² Crosby, *Measure of Reality*, 11, 17, 53, 81, 238.

²³ Samuel P. Huntington, *The Clash of Civilizations and the Remaking of World Order* (New York, 1996).

²⁴ See Roger Hart, “On the Problem of Chinese Science,” in *The Science Studies Reader*, Mario Biagioli, ed. (New York, 1999).

elsewhere we find “the West was making up its mind”).²⁵ Ultimately, the West becomes the fictional subject of a praise-and-blame civilizational hagiography, which narrates its trials, tribulations, and eventual triumph.

IN INTRODUCING THE NEW MODEL OF REALITY that forms the central thesis of his book, Crosby offers the example of Niccolo Tartaglia’s experiments with cannon balls: “He fired from a culverin two balls of equal weight with equal charges of powder, one at 30 and the other at 45 degrees of elevation. The first went 11,232 Veronese feet, the second 11,832.” Crosby exclaims: “This is quantification. This is how we reach out for physical reality, push aside its darling curls, and take it by the nape of the neck.”²⁶ The question is this: to explain Tartaglia’s experiment, do we really require recourse to an abstraction termed “the West,” endowed with a *mentalité*, shifting its perception of reality from the qualitative to quantitative? Or might we instead adopt a *deflationary* viewpoint, that Tartaglia—along with the majority of Italian mathematicians of the period—was in fact genuinely interested in measuring the flight of cannon balls? To explain the superiority that several European empires developed in weaponry, do we really require theses extolling the uniqueness of Western thought? Or might we instead (providing the same kind of historical analysis we offer for other conquests) explore a comparative history of weapons, a history in which the experiments of Tartaglia are but one part?

In *Measure of Reality*, Crosby proposes a “big-picture” thesis: “The West in the sixteenth century was unique”—its “New Model offered a new way to examine reality . . . providing humanity with unprecedented power.”²⁷ Without anything more than anecdotal evidence for changes in the extent of quantification, Crosby’s claim of a great shift from a qualitative Venerable Model to a quantitative New Model in the late thirteenth century is nothing more than yet another repackaging of discredited claims of a radical shift in Western thought. Without the technical analyses of the sciences of weapons and navigation, historical explanations for developments in these fields are impossible, and Crosby can only turn to claims of the efficacy of a special, newly discovered method of knowing reality. And without any examination of non-Western civilizations, comparisons are meaningless; the dramatic differences between the West and other civilizations that appear in Crosby’s book are simply the result of his failure to present information easily available from secondary sources about the non-Western civilizations. In sum: instead of deriving historical conclusions from empirical evidence, Crosby begins with an often-repeated mythology about the West, its radical shift from ancient to modern, its uniqueness among civilizations, and its ultimate triumph. To this, he adds a new set of explanations—the “Old Model” and “New Model”—for which he offers neither rigorous definitions, supporting evidence from primary historical documents, nor justification from secondary research in the history and philosophy of science.

The shortcomings of Crosby’s *Measure of Reality* are representative of the

²⁵ Crosby, *Measure of Reality*, 19, 60, 239, 10.

²⁶ Crosby, *Measure of Reality*, 11–12.

²⁷ Crosby, *Measure of Reality*, 238–39.

problems of an entire genre of historiography written in the twentieth century. In this literature, the imagined communities of “the West,” “China,” and “Islam” were taken to be the central protagonists in praise-and-blame histories of civilizations; simplistic teleologies of science provided universal benchmarks to measure the progress of civilizations toward modernity; the purported radical break between the ancient and modern in the West was transposed onto a fictive Great Divide between the primitive non-West and the modern West; the ignorance of the science of other non-Western civilizations was mistaken for ignorance of other civilizations of science. And in this literature, with the Great Explanandum—the known uniqueness of the West—as the given starting point, practically any study of language, thought, society, institutions, or politics could be called into service as the explanation of what makes the West unique. One task for critical history, then, must be to analyze the rhetorics, ideologies, and academic disciplines that authorized this particular genre of world history.²⁸

²⁸ For further discussion of these issues, see the web page for the conference “Rethinking Science and Civilization: The Ideologies, Disciplines, and Rhetorics of World History,” at www.stanford.edu/dept/HPS/RethinkingSciCiv/.

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Review Essays
Thinking Unfashionable Thoughts,
Asking Unfashionable Questions

MARGARET C. JACOB

FOR MY GENERATION OF HISTORIANS, who went to graduate school from roughly 1962 to 1975 (and hence now for many of the graduate students we have trained), the big questions in Western or world history became strangely unfashionable. None is bigger than the question of what were the factors that made Western hegemony possible. Indeed, the very notion of Western hegemony, of the domination of much of the world by Western political or economic power from roughly 1800 to 1970, may be said to be so fraught with anger or guilt as to be almost untouchable. In the present climate, even posing the question "Why the West?" conjures up more embarrassment or hostility than awed curiosity. Just asking it suggests the possibility that the questioner approves of the course that human history took from roughly 1500 to 1900. Admitting that the West did achieve hegemony seems almost tantamount to endorsing the means and methods used to achieve it. Suspicious critics might even presume that a Eurocentric questioner would dwell on Western strengths while ignoring the injustices associated with conquest or imperialism. In addition, seldom do specialists in non-Western histories—the great Joseph Needham, as always, an exception—dwell on the question. Indeed, as the work of Roy Bin Wong shows, specialists in Chinese or African history delight in raising the stakes by showing the relative insignificance of differences between their area and period and a comparable time in the West.¹

As a result of an embarrassment sprung from a multitude of sources—not least the rise of women's history—most historians with credentials in European or American history, or even the history of science and technology, have little to say about what is, arguably, the single most extraordinary phenomenon in global history. The silence in the history of science is particularly striking. Everyone who has thought about the question "Why the West?" acknowledges the singular importance of mechanical science, both as a mindset and in its application to manufacturing technology. Inventions from larger ships to the steam engine figure prominently in the histories of the Atlantic trade or the rise of Birmingham and Manchester. But most histories of technology now stop at that point and do not ask why those inventions occurred where and when they did. In addition, the history of science has been preoccupied with recovering the social, with situating science, and

¹ Roy Bin Wong, *China Transformed: Historical Change and the Limits of European Experience* (Ithaca, N.Y., 1997).

occasionally with quarrels about method, where terms like “social construction” are rallying cries.² Local studies—some of them of singular quality—have dominated the history of science broadly conceived.³ Since 1945 and beyond, when most American history of science programs were started, the discipline has matured and deepened its institutional foundations. But comparative studies—even within a Western framework—have been few and far between.

’Tis a pity. Because the question “Why the West?” has become unfashionable on the left—and that is where most of my academic generation situates itself—it has been consigned, broadly speaking, to the right, or to historians whose political discourse was shaped before gender, or the social, then the linguistic/cultural turn, came to prominence. Yet the big question about the rise of the West remains important, and in our own time there are still a few historians who defy fashion and try to answer it. Predictably, none of the inhibitions commonplace among post-1960s historians hamper the perspective of Alfred W. Crosby in *The Measure of Reality: Quantification and Western Society, 1250–1600*. He simply asserts that Western chauvinism has nothing to do with his enterprise, and he claims to know perfectly well that science and technology were “the edged weapon” of imperialistic expansion.⁴ The impulse that lay at their root—quantification—therefore becomes all the more interesting. Crosby wishes to understand how quantification unfolded and even to explain why it did. The impulse to measure and count, he believes, holds the best answer to “Why the West?”

Crosby’s endeavor has many noteworthy and positive features. He has mastered a vast English-language historical literature, both primary sources in translation and works by historians such as Samuel Y. Edgerton, Jr., Carlo M. Cipolla, and Elizabeth L. Eisenstein. He writes with admirable clarity and verve about the quantification of time, space, music, painting, even bookkeeping. He presents the Renaissance in the spirit of Jacob Burckhardt, and he gives the medieval its due. When available in paperback, the book would be superb for any Western Civilization course, readable by freshmen and sure to provoke discussion. It interrelates phenomena that tend to be studied in isolation: the invention of clocks in the 1270s and the reconceptualization of time, Ptolemy’s contribution to cartography, the metaphysical implications of mathematics—“mathematics is also glorious because in its generality it is powerful enough to tempt us to tackle the biggest mysteries”⁵—and the connections between musical theory and astronomy.

Most of these connections should be familiar to historians of early modern science and technology, even if they generally do not address the large questions that Crosby has set for himself. By the fifteenth century, Westerners had quantified and mechanized their societies, Crosby claims, far more than anywhere else on earth; and, “compared with contemporary Muslim, Indian, and Chinese civiliza-

² For a summary of where the quarrels now stand, see Ronald Giere, *Science without Laws* (Chicago, 1999). See also Margaret C. Jacob, “Science Studies after Social Construction: The Turn toward the Comparative and the Global,” in Victoria E. Bonnell and Lynn Hunt, eds., *Beyond the Cultural Turn: New Directions in the Study of Society and Culture* (Berkeley, Calif., 1999), 95–120.

³ See, for example, Harold Cook, *Trials of an Ordinary Doctor: Joannes Groenevelt in Seventeenth-Century London* (Baltimore, 1994).

⁴ Alfred W. Crosby, *The Measure of Reality: Quantification and Western Society, 1250–1600* (Cambridge, 1997), 5.

⁵ Crosby, *Measure of Reality*, 122.

tions, [Europe] was uniquely prepared to survive and even to profit from such an avalanche of change.”⁶ Why did the Italians, the French, and the Dutch pioneer the quantification of nature? In passing, Crosby hints that the West may just have been accidentally a somewhat freer place to think and write. Decentralized, “Europe has always had an attic room or at least a dry corner in the barn for émigrés . . . [T]he political and religious aristocracies of Asia and North Africa always ultimately united to keep the nouveau riche down.”⁷ Crosby’s answer to “Why the West?”—insofar as he has an answer—begins to sound suspiciously like that old workhorse, the rising bourgeoisie, commercially busy in cities where a relative autonomy and anonymity prevailed.

Crosby is much better at describing how quantification moved from field to field, sometimes with practice leading theory, than he is at tackling why the purported mental undergirding of Western hegemony evolved when and how it did. Part of the difficulty lies with chronology. Crosby has chosen to end his narrative around 1600. Only hindsight (which is always 20/20) could imagine that by 1600 all the elements had been assembled that would make Western hegemony possible. With the exception of the Spanish empire, where by 1600 hegemony was more a dream than a reality, it cannot be said that by that date “Europeans . . . were [better] able to organize large collections of people and capital and to exploit physical reality for useful knowledge and for power than any other people of the time.” Crosby implicitly acknowledges that their ability to master came later than did their ability to quantify, but then he asserts that “*in time*” the quantifying habit of thought would lead to swift advancement.⁸ Crosby never directly acknowledges that both widespread habits of mathematical thought and the *means* to organize people and reality were essentially developments of the seventeenth and eighteenth centuries. Only a simplistically Whiggish account could explain the intellectual and industrial developments of those two centuries solely by reference to the foundation that Crosby’s book attempts to lay for them.

Crosby’s book ultimately fails to answer the very question that it deems so central to its enterprise. The limitations of this account derive from more than simply those imposed by its own chronological limits. They have to do with the shortcomings of intellectual history classically conceived, with the holding of social and political reality firmly in the background, if present at all. While I have no idea where the author would situate himself generationally, his book can be situated by a method that predates the innovations that transformed intellectual history and the history of science during the 1960s. “The social” is jarringly absent from Crosby’s approach and hence his account. The late medieval people who made and lived with these new practices are strangely absent. We encounter intellects or ideas abstracted from their human setting. Predictably, no women figure anywhere in Crosby’s story about the rise of quantification.

The material and household culture wherein inventiveness occurred must be central to the story of any set of changes as relatively rapid and distinctive as Crosby claims. A new interiority that permitted change, yet allowed new practices to be

⁶ Crosby, *Measure of Reality*, 53.

⁷ Crosby, *Measure of Reality*, 54–55.

⁸ Crosby, *Measure of Reality*, 9, 10, italics in original.

reconciled with the old in some sort of personal harmony, never appears in Crosby's account either. René Descartes possessed that interiority, just as his *Discours de la méthode* (1637) spread it far and wide.⁹ Nor does it seem to matter that so many of the changes Crosby recounts occurred in the vernacular and not in Latin. The vernacular defined the female linguistic universe, while Latin remained an almost exclusively male medium. Household learning may have contributed as much as commerce to making quantification habitual and its range ever expanding. When we can peer into the lives of families where quantification had become habitual—the Watts of steam-engine fame spring to mind—there we find female literacy and numeracy, as well as energetic involvement in passing that education on to the next generation.¹⁰ Similarly, there is considerable evidence to suggest that literacy and numeracy spread more rapidly in Protestant and not Catholic Europe after 1600. Any account of the rise of the West that ignores the effects of the Reformation—as does Crosby's—stands remarkably out of touch with a historiography that begins with Richard Henry Tawney and includes Robert Merton, Christopher Hill, and Phyllis Mack.¹¹

In Crosby's account, the personae even of leading intellectual figures seem strangely remote. From the work of Frances Yates, we think we know the complexity of Giordano Bruno reasonably well. But in Crosby's telling, he becomes one dimensional, worth knowing when he said something deemed to be rational or progressive. Yates's Bruno, a somewhat crazed exponent of a new Copernican religiosity, has no place here.¹² Similarly, the great astrologer of Elizabethan England, John Dee, receives credit for his interest in mathematics, but Crosby tells us that "in spite of John Dee . . . the West . . . has produced most of the good applied physicists, engineers, and accountants." The John Dees of the late sixteenth century, even with their propensity to talk to angels, self-consciously sought the mathematization of all learning.¹³ The peculiar religiosity of the Dees is part of the *because*, not the *in spite of*, that Crosby is trying to understand. Beginning in the 1960s with the work of Frances Yates, historians of early modern Europe have rescued many of its leading natural philosophers from the rationalist account of their motives and interests. Crosby has not mastered that rich historiography.

Because familiar historical figures seem distorted, a skeptical meditation on the book's undocumented assertions about other cultures seems appropriate.¹⁴ The self-confidence of the assertions about non-Western cultures—with which Crosby

⁹ For an excellent discussion of Descartes' social and mental universe, see Stephen Gaukroger, *Descartes: An Intellectual Biography* (Oxford, 1995); compare Jonathan Dewald, *Aristocratic Experience and the Origins of Modern Culture: France 1570–1715* (Berkeley, Calif., 1993).

¹⁰ Margaret C. Jacob, *Scientific Culture and the Making of the Industrial West* (New York, 1997), chap. 6. I am aware of how difficult it is to find evidence for affective lives prior to 1700; that should not stop an author from looking for it.

¹¹ Richard Henry Tawney, *Religion and the Rise of Capitalism*, with a new introduction by Adam B. Seligman (1926; New Brunswick, N.J., 1998). See Robert K. Merton, *Science, Technology, and Society in Seventeenth Century England* (New York, 1970); Christopher Hill, *The Century of Revolution 1603–1714* (London, 1961); Phyllis Mack, *Visionary Women: Ecstatic Prophecy in Seventeenth-Century England* (Berkeley, Calif., 1992).

¹² Frances Yates, *Giordano Bruno and the Hermetic Tradition* (London, 1964).

¹³ Deborah Harkness, "Managing an Experimental Household: The Dees of Mortlake and the Practice of Natural Philosophy," *Isis* 88 (1997): 247–62.

¹⁴ See Crosby, *Measure of Reality*, 53–55.

seems to have little familiarity—complements an absence of nuance about the thought and lives of Europeans. Add to that void a tendency to make firm, solely modern distinctions between science and magic—separations foreign even to Isaac Newton¹⁵—and the reader in search of historicity begins to bristle. Could it be the case, after all, that just asking the big questions requires a slighting of the authenticity of one's subjects? That need not happen. Employing a more socially anchored history of ideas makes context more visible and allows the historian to relate new practices, whether in music or accounting, to their religious, educational, or ideological setting.

Without embarrassment, historians of every generation need to address the big questions because they are important and because people (like our students) inevitably ask them. We are all indebted to Crosby for his courage, even if we find fault with his methods or his argument. He may also be the harbinger of a historiographical shift. More global history is now being mandated in the high schools, in Virginia, New York, California, and Texas—for starters—and the best of the new curricula evince no awkwardness about big questions.¹⁶ In a few years, many of our students will come prepared, insofar as possible, with high school courses that were not afraid to address large, globally focused questions. From their ranks may arise a new generation willing to ask, "Why the West first?" and to do so from a more global and nuanced perspective. Will Crosby's answer be sufficient? Employing his own framework of broad generalizations, here, too, the answer I must give will be "no."

The impulse to quantify was singularly important to what we may call, for shorthand, the Commercial Revolution. This information leads only to the conclusion that all of Europe might have remained like the Dutch Republic. Europeans would have been immensely skilled at striking out in sleek ships and trading and keeping track of everything from goods to slaves. Like the Dutch, they may have sustained outposts and plantations, but probably they would have been forced in the end to jostle with local trading competitors who could imitate Western methods, devise new ones, and reclaim their markets. Or, in another version of Crosby's scenario, Italy and France would have remained in 1700 the intellectual powerhouses that he depicts them as having been in 1600. But who, then, would have industrialized? Advanced mathematics of the kind so brilliantly practiced in the French academies after 1700 did not an industrial revolution make.¹⁷ Having the skills to be commercial affords no guarantee of being technologically industrial, in the post-1760s British meaning of the term. Witness that the Netherlands did not industrialize until well into the nineteenth century. Indeed, Crosby's account, precisely because it stops in 1600, gives no attention to the crucial social and political settings of the seventeenth and eighteenth centuries. State formation and

¹⁵ Betty Jo Teeter Dobbs, *The Janus Faces of Genius: The Role of Alchemy in Newton's Thought* (Cambridge, 1991).

¹⁶ *History-Social Science Framework California Public Schools 1997 Updated Edition* and *Texas Essential Knowledge and Skills (TEKS)*, as upgraded at their respective web sites. For a survey of where various states now stand, see David W. Saxe, "State History Standards," Thomas B. Fordham Foundation, *Fordham Report* (February 1998), vol. 2, no. 1, www.edexcellence.net/fordham/foreports.html.

¹⁷ For a development of his point, see Jacob, *Scientific Culture*.

systems of government played key roles in the economic developments that appear after 1750.

Without the political and intellectual transformations associated with the English revolutions of 1640 and 1688, and taken up in turn by continental enlightened circles, little of the economic power represented in the concept “Western hegemony” would have been imaginable.¹⁸ Absolutist regimes put their scientific academies and their engineers under the control of both state and aristocracy. German elites who could quantify just like anybody else fought the introduction of railroads well into the mid-nineteenth century.¹⁹ Corporate structures, legal protections, local privileges, military, as opposed to civil employment for engineers, in the absolutist states, inhibited development—never mind hegemony—in vast sections of continental Europe including France. Decentralization in the highly urbanized Dutch Republic allowed all sorts of immigrants and publishers to find a haven, but it also permitted about two hundred families, who made their fortunes largely in commerce, to control local institutions right up to 1848. By that time, the Dutch had been driven out of the race for imperial dominance, casualties of their small home market and their failure to modernize socially and industrially.

If there was an epicenter where hegemony was first consolidated, it was England after 1660. Agricultural surpluses combined with commercial aggressiveness put England on the world economic map in ways that contemporaries noted. But even that dynamism would have been difficult to turn into an industrial and hegemonic transformation without the securing of parliamentary and constitutional institutions after 1689. Having a centralized system of government that could be coaxed to favor commercial interests created a new infrastructure of roads and canals. They undergirded industrial expansion in the period after 1760. Similarly, the system of non-Anglican education that emerged in Britain after the Act of Toleration meant that there were dozens of academies for young men over fourteen where scientific learning and its application could be accessed with relative ease.²⁰ Scottish universities were similarly innovative. By comparison, the French colleges remained in the grip of clerical masters—25 percent of whom were Jesuits—throughout the century, and technical education entered the French curriculum in earnest only after the 1760s. By that time, the French ministers in charge of fostering manufacturing had developed a veritable obsession with British technological innovation. A generation later, a French spy writing home to the French revolutionary ministers said it all: “[When traveling in England] I saw with dismay that a revolution in the mechanical arts, the real precursor, the true and principal cause of political revolutions was developing in a manner frightening to the whole of Europe, and particularly to France, which would receive the severest blow from

¹⁸ Compare Larry Stewart, “Glorious Industry: The Revolution of 1688 and Technological Change in England,” *Annals of Scholarship* 5 (1988): 415–38.

¹⁹ Eric Dorn Brose, *The Politics of Technological Change in Prussia: Out of the Shadow of Antiquity, 1809–1848* (Princeton, N.J., 1993).

²⁰ For the most recent contribution to the history of science education in Britain, see David A. Reid, “Science and Pedagogy in the Dissenting Academies of Enlightenment Britain” (PhD dissertation, University of Wisconsin, Madison, 1999). The story was nowhere near as open for women, but note the total absence of mathematical learning for French girls; see Martine Sonnet, *L’éducation des filles au temps des Lumières* (Paris, 1987).

it.”²¹ The spy Le Turc did not even think about the global implications of what he saw. But we should.

Keys to answering the big question lie also in the distinctive types of scientific cultures that emerged by 1700. Crosby privileges an exclusively intellectual, scientific aesthetic and in general slights its technical and mechanical fraternal twin. His method offers no direct path to the consolidation of modern science from chemist Robert Boyle to physicist Isaac Newton, nor any path whatsoever to the scientific culture that emerged first in Britain. Based on Newtonian mechanics and housed in voluntary societies like the Spalding and the Lunar Society, scientific sociability appeared in London and the provinces after 1700. While French science, when finally Newtonian, remained conceptually and mathematically innovative, British science turned increasingly toward the applied. In his emphasis on invention, James Watt was as typical of British scientific culture as polymath Jean d’Alembert was of its French counterpart.

If Crosby’s exclusively mathematical explanation for the “rise of the West” were deemed sufficient, the course of world history after 1600 might have been vastly different from what in fact happened. Westerners might have remained the great quantifiers and measurers, counting their bundles or making maps for some other great empire. So, too, if Crosby’s method of pinpointing ideas devoid of their setting were the only one open to us, then the actual people who used clocks, educated their children with an eye to the practical, bought the new art in Antwerp or Amsterdam boutiques, measured and weighed, would be lost to us forever. He has found a piece of the Western story and told it to good effect. It should not be presumed to be the whole, or even half, of what remains to be said.

²¹ Conservatoire des Arts et Métiers, Paris, MS U 216, Le Turc to Citoyen, 14 Nivôse An 3 [December 1794]. I owe this reference to the kindness of the late J. R. Harris. See J. R. Harris, *Industrial Espionage and Technology Transfer: Britain and France in the 18th Century* (Aldershot, Hampshire, 1998).

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Review Essays
Whose Measure of Reality?

JACK A. GOLDSTONE

MODERN SOCIOLOGY AROSE in the mid-nineteenth century. Western Europeans such as Auguste Comte, Alexis de Tocqueville, Henri de Saint-Simon, John Stuart Mill, and Karl Marx sought to understand what they perceived as a radical transformation of society. Those twin whirlwinds, democracy and industrialization, seemed to have created an entirely new form of society, distinct from the aristocratic agrarian societies that had been typical of all civilizations around the globe for the previous five thousand years.

Today, there is still a great gap—in incomes, in lifestyle, and in governance—between the societies that spawned early sociology and the rest of the world. Some non-European societies have caught up with the West and become democratic, urban, and industrialized. But most of the nations of Africa, Latin America, the Middle East, and Asia still are lacking in one or more of these characteristics; if we add up those people who remain either under authoritarian rule, bound to rural life, or with pre-industrial levels of income, they would be the majority of humankind. This puzzle—in the words of David S. Landes's title, "why some are so rich and some so poor," remains central to understanding the condition of the world today.¹

Until recently, the answers to this question have been of two kinds. One answer is that, for a long time, the Western nations had been better—more industrious, more skilled, more inquisitive, graced with a better mix of natural resources, more disciplined—than other nations in ways that mattered for economic growth; this is the argument that Landes presents. The other answer, from the Marxist tradition, is that Western nations and their elites had been more aggressive, more rapacious, and more effective at subjugating other peoples and climbing up over their enslaved and exploited bodies. Either way, the great divergence is due to something the West had that the rest of the world lacked.

The problem in these approaches, however, is that no identification of that "something" has proved wholly convincing. On close inspection, most characteristics alleged to give a special character to the West turn out either to be found in non-Western nations too or to have no clear causal connection to the innovations in constitutions and production processes that were the key elements in creating Western modernity. For example, clock-making skills (by which Landes places great

¹ David S. Landes, *The Wealth and Poverty of Nations: Why Some Are So Rich and Some So Poor* (New York, 1998).

store) can be found in both China and Italy in late medieval times. But in neither region did industrialization emerge except by late importation from northern Europe. Cultural characteristics long thought to doom Asians to pre-capitalist life, such as Confucianism, are now seen by some as ideal foundations for advanced industrial organization. Western Europe's conquest of the New World, once seen as unique, may have had counterparts in the Russian expansion into central and eastern Asia, and in the Qing expansion into central and southeastern Asia. (And, for that matter, the countries that moved most effectively into the New World, Portugal and Spain, showed the least modern characteristics in Europe right up to the early twentieth century!) Failures of the theory of the West's "special something" have led some scholars toward yet a third hypothesis—that there was in fact no special something that fated the West for superiority over other civilizations, and that its divergence occurred relatively late (not until the late eighteenth century) and largely as a concatenation of chance circumstances.²

Alfred W. Crosby enters this debate with one of the most lively, entertaining, and consistently fascinating books on the origins of the West's modern advantage over other civilizations. Crosby's innovation is to search for the "something special" in Europe further back in time than most others have done, and in a particular corner of European culture. Looking back to the late Middle Ages, Crosby argues that, circa 1300, western Europeans developed an obsessive fascination with subjecting their world to uniform measurement, dividing everything into greater or fewer uniform units; that this uniform division of time and space, of music, and of visual representation (whereby the space on maps and paintings was laid out by rules of perspective) was unique to the late medieval and early modern West; and that this pattern laid the basis for the West's later domination of the world.

Crosby has already given us a superb account of one dimension of Europe's rise—the uncanny ability of Old World flora and fauna to drive out competitors in the New World, and thus to create a "new Europe" in the Americas.³ Here, he moves on to consider what set western Europeans apart from other civilizations in the Old World, and thus to complete his account of the underpinnings of the West's global triumph in the nineteenth and twentieth centuries. I am such an admirer of Crosby's earlier work, and of the wit and verve of this volume, that it pains me greatly to say that, while I agree with some reservations with his first two arguments listed above, I find the argument that quantification per se was the key to Western global domination to be wholly unwarranted.

Let us begin with the parts of Crosby's argument that are easy to accept. First, the western Europeans of the late thirteenth century were by no means the first or foremost virtuosi of measurement, as Crosby concedes. The ancient Greeks, notably Eratosthenes (276–194 BC), made measurements of the size of the Earth

² Jack A. Goldstone, "The Problem of the 'Early Modern' World," *Journal of the Economic and Social History of the Orient* 41 (August 1998): 249–84; Andre Gunder Frank, *ReOrient: Global Economy in the Asian Age* (Berkeley, Calif., 1998); Kenneth Pomeranz, *A Great Divergence: China, Europe and the Making of the Modern World Economy* (Princeton, N.J., 2000).

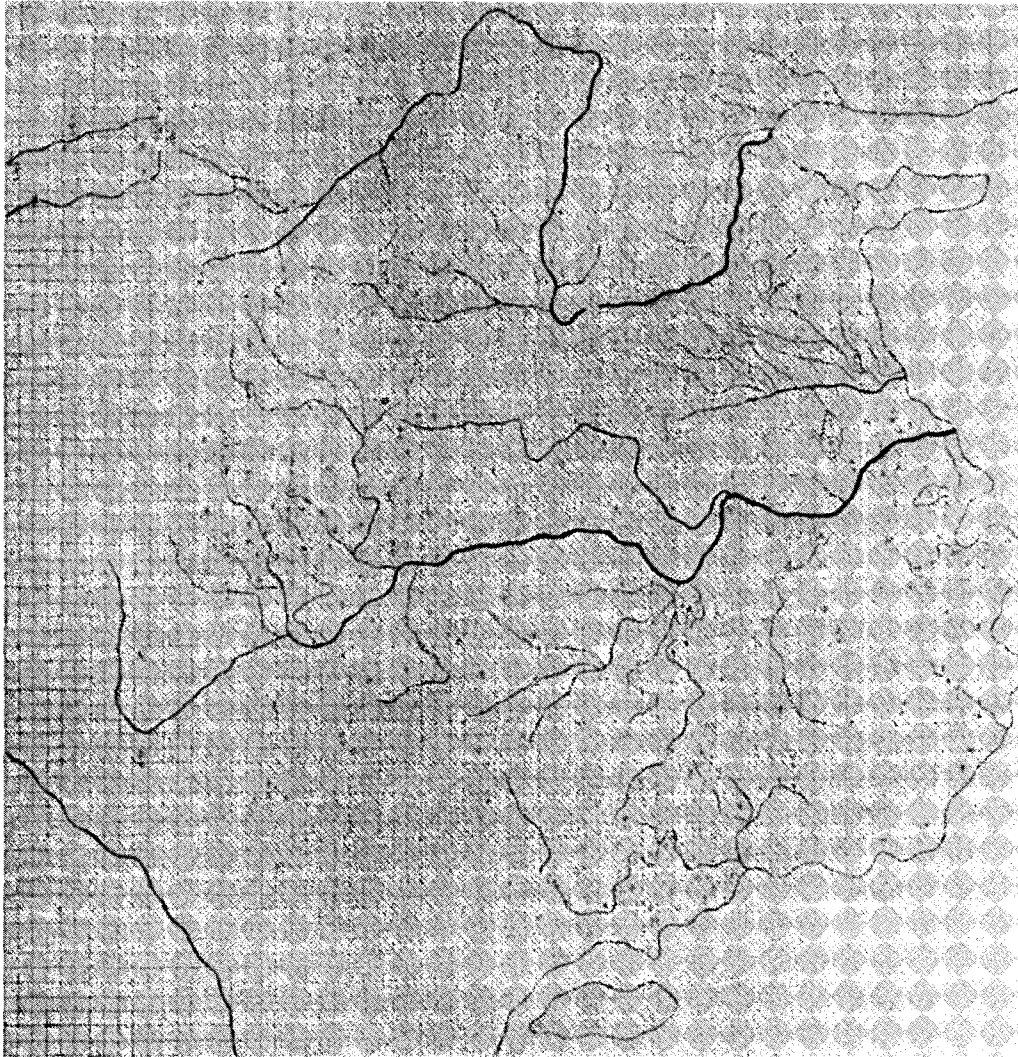
³ Alfred W. Crosby, *Ecological Imperialism: The Biological Expansion of Europe, 900–1900* (Cambridge, 1986).

and distances in the solar system. The Greeks were obsessed with arithmetic and geometry (Plato made them the true basis of reality); they developed algebra (Diophantus, c. 250 AD, developed symbolic notation and solved equations for unknowns) and immersed themselves in the study of uniform shapes (circles, ellipses, and regular polyhedrons). The Greeks also knew of the relations of musical harmonies to arithmetic ratios, and found in that relationship a proof of the preternatural power of numbers. Of course, they made little practical use of these measurements outside of architecture, but then neither did the late medieval or Renaissance scholars who revived their work. The Arabs of the tenth century AD could also boast notable arithmeticians and astronomers, such as Abu 'Ali al-Hasan ibn al-Haytham (965–c. 1041 AD), whose theory of light as straight emanations and elaborations of Euclid's geometry influenced Johannes Kepler and René Descartes. And of course, it was the Arabs who transmitted (and perhaps developed?) the system of notation we know today as Arabic numerals. The Chinese developed massive mechanical clocks some years before Europeans did, and their abacus remained a superior means of calculating sums for many centuries compared to Europe's counting boards.

But where the Europeans do seem different from other civilizations is their willingness to subject everything they cared about to the discipline of measurement. Other civilizations knew of clocks, but they did not regulate time in their cities or temples by the movement of ratchet and escapement. Other civilizations made maps and accurately charted the movements of the heavens, but they did not strive to reduce everything to uniform grids or uniform motions. Other societies conducted detailed surveys of their lands and conducted business through weights and measures and monetary prices, but they did not model the universe as a mechanism or clockworks.

Why did western Europeans replace a vague, religious hierarchy-based view of natural reality with a view based on uniformly divided time and space, over the course of the thirteenth to sixteenth centuries? That the change did occur seems undeniable, and Crosby documents it brilliantly, ranging over Europe's efforts to measure time, space, musical rhythms and scales, and representational space. But Crosby stumbles a bit in explaining why this change occurred, and even more in attributing to it something besides a revival of past knowledge and importation of Arab and Eastern insights.

Crosby attributes much of the West's fascination with numbers and uniformity to something that is completely unwestern in its provenance—the prevalence of merchant culture. Completely unwestern not because the West lacked for merchants but because Western merchants were entirely unexceptional by standards of the world around 1300. Throughout southeastern Asia, indeed, the entire Indian Ocean basin, a complex system of urban trading centers that was engaged with many different currencies and nations had developed by this time. As late as the seventeenth century, European merchants traveling to the East were overwhelmed by the fabulous wealth of their Indian and Chinese counterparts. Japan and China were able to fully control and deny Western merchants who sought to trade with



Chinese mapmakers produced remarkably precise and uniformly gridded maps long before Europeans, and continued to produce them for several centuries. This plate is from *Science and Civilisation in China*, by Joseph Needham and Wang Ling, Vol. 3: *Mathematics and the Sciences of the Heavens and the Earth* (Cambridge, 1959), figure 226. This example, the "Map of the Tracks of Yü the Great," a carving on a stone tablet from 1137 AD currently in the Pei Lin Museum in Xian, China, is an outstanding example of China's uniform grid cartography. Each square is scaled to an area of 100 *li*. The paths of the Yellow and Yangtze rivers and their tributaries are shown with extraordinary accuracy. The stone is about 3 ft. square. The geographer is unknown. Reproduced by permission of Cambridge University Press.

their hinterlands. Even in their own societies, Western merchants remained a relatively weak force compared to Western bankers (Fuggers and Medicis, for instance) and landed nobles throughout the period that Crosby examines.⁴

Crosby seems to suggest a special "merchant" culture developing from the late

⁴ Janet Abu-Lughod, *Before European Hegemony: The World System A.D. 1250–1350* (New York, 1989); Roderich Ptak and Dietmar Rothermund, eds., *Emporia, Commodities, and Entrepreneurs in Asian Maritime Trade, c. 1400–1750* (Stuttgart, 1991); Jan Wisseman Christie, "Javanese Markets and the Asian Sea Trade Boom of the Tenth to Thirteenth Centuries, A.D.," *Journal of the Economic and Social History of the Orient* 41 (August 1998): 344–81.

Middle Ages through the Renaissance by contrasting the worldview of European nobles (lavishly generous and careless of their wealth, and placing the “honor” of bravery and warrior skills above the “market honesty” of scrupulous accounting) with the worldview of the urban merchants (attentive to every penny), and then further suggesting that all other societies had values closer to those of Europe’s nobles. Thus other cultures, lacking the detailed attention to accounts and money and debt that characterized Europe’s merchant class, could never develop the fascination or skill with measurement of Europeans. But this is balderdash, and comes of comparing apples and oranges. Yes, European merchants had a different worldview than European nobles, or Chinese literati, or Ottoman sultans. But what about comparisons with non-European *merchants*? Everything we know about the merchants of the Middle East, India, and China suggests that they were just as attentive to accounts and profits, just as concerned about measuring their wares, as their European counterparts.⁵ The merchants of Cairo and Baghdad, whose outlook differs so dramatically from that of the warriors and princes of Islam, or the merchant-commoners of Osaka who held the fortunes of the samurai elites under their control in late Shogunal Japan, similarly distinguished themselves from the warrior elites of their societies. Crosby takes a well-known difference—between outlooks of warrior elites and business elites—and falsely turns it into a difference between Western and non-Western civilizations. Until at least the late eighteenth century, Western urban businessmen and merchants had no greater prominence in their societies than the salt merchants of China or the bazaar merchants of the Middle East and North Africa.

Crosby suggests that an extraordinary desire for wealth and an extraordinary striving to gain bullion is what drove Europeans to seek gold in the New World. He goes so far as to say that a shortage of bullion made Europeans especially mad to obtain it.⁶ But surely this implies no European virtue or advantage. The relative weakness of European merchants and their being shut out from the most lucrative trading areas of the East propelled Spanish and Portuguese adventurers onto the Western oceans. Moreover, those societies most deeply involved in the exploration for, and exploitation of, foreign bullion (Spain and Portugal) are noteworthy particularly for the *absence* of any contribution they made to the advance of mathematical or technical prowess in Europe after about 1600. If there was any nation in the world “mad” to obtain silver bullion, it was not in Europe but in China. Europeans happily turned over all the bullion they could find to Asian (mainly Chinese) merchants in exchange for spices, silks, porcelains, and other goods more avidly sought in Europe. From the sixteenth to the eighteenth

⁵ Ptak and Rothermund, *Emporia, Commodities, and Entrepreneurs*; Karl Reinhold Haellquist, ed., *Asian Trade Routes, Continental and Maritime* (Copenhagen, 1991); Klaas Veenhof, “‘Modern’ Features in Old Assyrian Trade,” *Journal of the Economic and Social History of the Orient* 40 (November 1997): 336–66; Sanjay Subrahmanyam, *Merchants, Markets, and the State in Early Modern India* (Delhi, 1990).

⁶ Alfred W. Crosby, *The Measure of Reality: Quantification and Western Society, 1250–1600* (Cambridge, 1997), 73–74.

centuries, China was the world's great "bullion sink," and Europeans acted mainly as intermediaries in the transfer of bullion from the Americas to eastern Asia.⁷

Crosby commits an error all too common among scholars seeking the origins of European exceptionalism. They find some change or turning point in European history; assert (usually by only the most cursory comparison) that no similar turning point occurred elsewhere; and then conclude that the European global dominance of 1850 can be attributed to that turning point, since they have shown that it was "uniquely" European. The logical problems in this approach should by now be obvious.

First, European history might well have unique turning points that simply moved Europe from an "odd" point (the Dark Ages of disconnect from classical roots after the collapse of the Western Roman empire, or the slowdown of trade after the Black Death) back onto a path that was actually common around the world, such as the revival of classical learning or the expansion of trade. Second, a more detailed or extensive examination of non-Western history could well show that supposedly "unique" elements in Europe did have parallels elsewhere. For example, Chinese cartographers created superb grid maps, and Chinese cities often had a regular grid layout in their streets, while most European towns still showed the mangled byways and disorder characteristic of the Middle Eastern bazaar. Finally, even if a unique element of European experience is shown (as for example, the use of grids to create realistic perspective in painting), this hardly suffices to demonstrate that this unique element is connected to European superiority. That is, to the Chinese of the seventeenth century, the fact that the Europeans had nifty clocks, or realistic paintings with vanishing points, made no great impression. These things no more signified European present or future superiority than Roman baths or Irish illuminated manuscripts (also admirable and without exact counterparts in the East). And although the line from, say, perspective in painting to steam engines may seem straightforward, this is an accident of hindsight, easily corrected by noting that the France of Descartes and the Italy of Galileo not only got the physics of the solar system wrong, putting their faith in vortices instead of centrally attracting gravitational force, but were also late to develop and adopt the steam engine and factory manufacturing, compared to the far more empirical British. If China had been able to develop a steam engine, drawing on its centuries of experience with pumps through its complex irrigation works and its superb metallurgy, we might have looked back on Europe's obsession with "uniform" space and time as a Platonic dead end that distracted Europeans from empirical trial-and-error tinkering and pragmatic inventions.

Therein lies the biggest weakness of Crosby's beautiful effort—he is so concerned to demonstrate threads of continuity in European intellectual history that he entirely misses abysses of discontinuity. Although both thirteenth-century logicians and seventeenth-century natural philosophers counted, measured, and struggled with the legacy of Aristotle, the medieval logicians sought to refine the classical

⁷ Dennis O. Flynn, *World Silver and Monetary History in the 16th and 17th Centuries* (Aldershot, Hampshire, 1996).

master, while Galileo and his followers sought to refute him. Moreover, Crosby's intellectual history stops several generations before the Industrial Revolution, assuming there is nothing at all problematic in going from Galileo's simple mechanics to the complex engines of Richard Arkwright, James Watt, and Robert Fulton. Yet if the progression from Galilean thought to industrial prowess is so smooth, how is it that Italy—home to the high Renaissance, precocious urbanization, and merchant dominance—lagged even the Scottish lowlands in economic development?

Using Crosby's argument, one could well compare medieval Islam and *early* medieval Europe and tell the reverse story: Given that the Arab world was so advanced in arithmetic and astronomy, and possessed the classical heritage of ancient Greek geometers, algebraists, and astronomers lost to Europe, which had fallen into a Christian mysticism; and that the Arab world was so obsessed with geometry and language (as shown in their art while the West indulged in spiritual display in its cathedrals); and that Arab merchants were far more enterprising than Western peddlers, the former cultivating commercial empires extending from Africa to India—well of course the Arab civilization must, a few centuries hence, be the dominant scientific and economic civilization of the globe. World civilization has had many periods of efflorescence that led nowhere: the Egyptian New Kingdom, classical Greece, the "Industrial Revolution" of the early Sung in China, the Caliphate of Baghdad, even the "Golden Age" of Holland; all seemed to put their societies on a path to world leadership, only to be outdone by a "tinkering society" on the remote British Isles in the nineteenth century.

The kinds of inferences that Crosby offers are intriguing but entirely unwarranted. If we are to determine what made the West more successful at producing things and designing weapons than other societies, we had better look closely at the history of agricultural production and technical invention, and compare it in substantial detail with other societies. We cannot simply set aside crucial sociological and political changes that were unique to the West, such as the movement toward equality and democracy that emerges at the same time as industrialization. To find a single event or trend that seems noteworthy in Western history, and assume all later difference between the West and non-Western civilizations flows therefrom, is to commit the kind of logical error that even Crosby's medieval Schoolmen would have frowned upon.

Crosby, with greater beauty, brevity, and delight than anyone I know, tells the story of a turning point that was surely necessary for the rise of the West; namely that, between 1200 and 1650, western Europeans emerged from a relatively mystical, hierarchical, and primitive or barbaric view of natural reality, rediscovered their own classical heritage and the advanced mathematical knowledge of the Orient, and so managed, fairly quickly, to "catch up" to the rest of the world. In the next two hundred years, they would surpass the rest of the world in mathematics, physics, the technology of production and transport, and the means of war. But how was the latter accomplishment tied to the former? If it was the sudden intoxication of a "rush" of new knowledge that impelled Europeans to push the borders of

knowledge, then it was perhaps not the quantification of reality itself but the centuries spent without the comfort of Eratosthenes and the Greek astronomer Aristarchus that prepared Europe for its leap forward. Or perhaps it was something else yet unknown. In any event, Crosby's charming and beguiling volume gives us one of the necessary causes for Europe's rise, but not a sufficient one.

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Reviews of Books

METHODS/THEORY

JO MURPHY-LAWLESS. *Reading Birth and Death: A History of Obstetric Thinking*. Bloomington: Indiana University Press, 1998. Pp. 343.

This book is not a history of childbirth but an examination of specific historical issues in childbirth with relevance to the present day. Written by Jo Murphy-Lawless, a feminist sociologist, the focus is on the nature of obstetric thinking. It is not always an easy book to read: it veers from the present to the past and back again; it makes few concessions for those who are not familiar with the general history of childbirth; and it does not always provide the broader context with which historians in general inform their work. Nevertheless, it is a stimulating book and raises significant issues for all interested in the history of medicine and science.

As with much feminist scholarship on obstetrics, an underlying concern is the perceived lack of participation of women in "managing" childbirth and the control of obstetrics by male practitioners. Ignored by most of the literature in this genre is whether the birthing woman had much control over childbirth even when midwives dominated. Murphy-Lawless goes beyond the traditional documentation of this narrative and addresses how male practitioners were able to gain control and maintain it. She does so by examining Irish medical texts from the eighteenth century onward and the ways in which their authors wrote about childbirth. For example, she discusses the medical perception of woman's body as unchanging, despite the reality of its ambiguous and very much changing nature. Medicine, through its ontological approach, made the latter into the former, in part, as a way of incorporating the "management" of childbirth into a scientific paradigm. Male midwives created a "science" out of theory and observation, unaware of the ideological context in which the observer and the observation existed. Underpinning theory was the gathering of statistics. Such a process made practitioners feel confident in what they were doing, even though the efficacy of their actual care did not always substantiate that confidence. One example of the process was dividing labor into several stages. As a result, Murphy-Lawless argues that the individual action of the woman became subsumed

into average times calculated for each stage—an act closely linked to the development of industrial time and time management concepts.

Using a case-study approach, Murphy-Lawless examines medicine's use of death through the issue of puerperal fever. She traces how the ideology of woman's frailty and incompetence played a role in determining what "medical" observations occurred and how practitioners interpreted them. Indeed, she maintains that this view of women was one of the reasons for the delay in making the link between practitioners' actions and women's deaths. The profession as a whole could adopt concepts of antisepsis and asepsis in wound management and, in doing so, extend the boundaries of what surgeons could do. Within obstetrics, however, the focus on woman's limitations directed failure to the patients and not to what practitioners were doing.

The most exciting part of the book is Murphy-Lawless's examination of the concept of risk. In our society and in medicine, risk is a statistical concept and, as such, it does not and cannot deal with real people and what they face; yet we embrace statistics with a rather non-scientific value: faith. Through the concept of risk, practitioners "read" the female body. Murphy-Lawless argues that the desire and need for medicine to speak with authority, to gloss over its internal disagreements, and its inability to live up to the ideal it created for itself, led to the centrality of risk in its practice. But in accepting and using risk, practitioners accept death as linked not to an individual body but to causes predicted through "laws" of probability. Risk is dependent on measurements that in the natural world are imposed and used as a reflection of the "laws" of nature. Risk is also tied to that equally nebulous term, "normality."

What needs to be addressed in future research is how medicine perceived and treated the male body. How much of what is being described is gendered? We just do not know, although the fact that obstetric practitioners were male and their patients female is clearly an issue and an underlying assumption in many studies. Research should also examine the pressures on practitioners to address the uncertainty of what they did (and still do) and the means they used to cope with it. My sense is that the concept of risk is part of

that response, as is the overwhelming attention paid to information gathering.

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JOHN C. BURNHAM. *How the Idea of Profession Changed the Writing of Medical History*. (Medical History, Supplement number 18.) London: Wellcome Institute for the History of Medicine, 1998. Pp. xi, 195. \$50.00.

John C. Burnham argues that during the past generation "professionals moved to the center stage" (p. 1) as both subjects and authors of the history of medicine in Europe and North America. His purpose in writing about the historiography of this field since the seventeenth century is to "illuminate the general historical process of change and innovation in ideas" (p. 3) as well as broader "historiographical issues at the end of the twentieth century" (p. 10).

Burnham displays impressive erudition in several languages in explaining why, until recently, historians "so often missed . . . not the existence but the power of the idea of profession among physicians of the past" (p. 182). Most of the book describes how, during the last half century, historians of medicine, "with the help of interested general historians, [have] reclaimed from sociologists the idea of profession as part of the history of the actual work of physicians, of physicians as experts, of physicians as social and economic figures" (p. 157).

This interpretation of the historiography of medicine is not entirely convincing, for three reasons. First, Burnham oversimplifies general historiography in the first third of the book. For example, he vaguely claims that historians' interest in both expertise and social reform in the early twentieth century was "typical"; the former of the "organizational, bureaucratic society of that period," the latter of "many thinkers early in the century" (pp. 37, 46). Moreover, he does not inquire if historians of medicine were any more or less indifferent to the idea of professions as collective historical actors than, for example, their peers who studied the history of law, religion, or art.

Second, Burnham dismisses evidence that challenges his conviction that historians of medicine were, until recently, isolated from other historians and social scientists. For instance, he overrules rather than argues against evidence that a substantial minority of historians before the late twentieth century wrote about the medical profession as more than merely a "collective noun," the "sum of the individuals who practiced it" (pp. 46–50). Several times he dismisses contrary evidence by pulling rank: insisting that "altogether" his interpretation that medical historians had a narrow focus is correct (e.g. pp. 56, 59).

Third, Burnham emphasizes the mutual influence of sociologists and historians of medicine but ignores the contributions of political scientists and economists to the historiography of medicine. He does not, for example, mention such influential concepts as interest

groups, regulation, political institutions, markets, production, supply and demand, and public goods. He writes as if "power," "bureaucracy" and "economy" have salience only among historians and sociologists (e.g. pp. 179, 181). Moreover, he does not acknowledge the profound influence of scholarship in economics and political science on some of the social scientists whose work on the history of medicine and health care he respects (for example, Paul Starr, Brian Abel Smith, Rosemary Stevens, and the author of this review).

Burnham chose twenty-eight images to illustrate the book. Twenty of these are portraits in various media made between 1652 and 1985. The others he labels "illustrations with a subtext of profession" (p. ix). However, he explains neither the purpose of these images nor why some of the photographs of his contemporaries are formal and others informal.

Not for the first time in a distinguished career, Burnham writes simultaneously as a disciplined historian and a polemicist. In one previous book, he blended incisive intellectual history with polemic about the ideals of scientists (*How Superstition Won and Science Lost: Popularizing Science and Health in the United States* [1987]). In another, he described the history of American vices in an ironic attack on indulgent liberals (*Bad Habits: Drinking, Smoking, Taking Drugs, Gambling, Sexual Misbehavior and Swearing in American History* [1993]).

Similarly, this book celebrates the triumph of Burnham and other historians of medicine over the narrowness of their predecessors as well as more than a few of their contemporaries. For Burnham, the consensus among scholars that a "collective sense of exceptionalism existed among physicians" and that the "sense of being a professional had profound social effects on patients and societies" is evidence of the success of historians' own professionalism (p. 184).

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JOHN T. GRAHAM. *Theory of History in Ortega y Gasset: "The Dawn of Historical Reason."* Columbia: University of Missouri Press. 1997. Pp. xix, 384. \$44.95.

This is the second of three volumes devoted to José Ortega y Gasset's thought. In the first, *A Pragmatist Philosophy of Life in Ortega y Gasset* (1994), John T. Graham considered Ortega's philosophy of life (his vitalism). A projected third volume will deal with his philosophy of language (a "new philology"). In this volume, Graham presents Ortega's philosophy and theory of history.

Graham warns us at the outset that he is "an intellectual historian of the 'old' persuasion" (p. xvii) and does not write intellectual history in the "new style" represented, for example, by Dominick LaCapra and Hayden White (p. xvii and p. x, n. 1). And indeed, what we have is a traditional, competent, and detailed reconstruction of Ortega's thought in the history of ideas mode. Graham patiently leads us through the

labyrinth of Ortega's basic concepts and traces the origins, developments, and variations of his ideas. But since Graham's purpose is less to "explain" Ortega's thought than to "comprehend" it (p. xviii), we remain for the most part within Ortega's texts and seldom venture out into the surrounding world in which Ortega lived, worked, and wrote.

The strongest aspect of Graham's book lies in its summaries, paraphrases, and quotations of Ortega's works. His reconstruction of Ortega's thought about history suggests its relevance to the concerns of our own historical moment, but Graham does not analyze this relationship. This raises the question of whether we might not do just as well to read Ortega's own works rather than try to grasp them through an intermediary.

The most useful aspect of Graham's book consists of his presentation of Ortega's historical theory and his reflection on the past in general. Here Graham succeeds in presenting Ortega as a source for renewal in historical thinking during our "weak moment" of historical consciousness. Ortega's writing about the past can be understood as a critique of culture whose primary concerns at present are problems of identity and subjectivity or the relationship among history and politics, ideology, and ethics. His pragmatic view of history, together with his method (historiology or metahistory), conceptual instruments for historical interpretation (generation, crisis, modernization), and eclectic attitude toward other scholars' ideas, are worthy of consideration. Ortega attempted "to encourage historians to try to view history from 'within' [from inside human life]" (pp. 274–75). He demanded a "radical historicization" of all concepts relating to human life (p. 140), advocated presentism ("the past is the history of the present" [p. 141]), and insisted that human identity could not be "fixed" (p. 145). All of these ideas are as fruitful and inspiring as his existential thinking and his theory of historical crisis, his idea of "historical rhythms" (masculine and feminine), and especially his concept of generation, which he considered as the most important in history.

Graham points out that Ortega was one of those thinkers who believed that the modern age was coming to an end and that the "postmodern" era had already begun by 1914. He was famous for his theory of world history, his concept of modernization theory, and his "prophecies" about the future of Europe. And even if Graham (following Hans Meyerhoff) sees Ortega together with R. G. Collingwood as a cofounder of a critical philosophy of history, Ortega's willingness to speculate remains an important feature of his thinking, since, as LaCapra used to say, "history without speculation is dead."

In an appendix that ends this book, Graham includes Ortega's text "Hegel and Historiology" (1928), wherein we read: "What counts most in man is his capacity to be unsatisfied" (p. 335). And this is what this reviewer feels on reaching the last page of Graham's book: dissatisfaction. Although Graham's ana-

tomical study helps us to recognize the importance of particular fragments of Ortega, the whole remains elusive.

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ROY ROSENZWEIG and DAVID THELEN. *The Presence of the Past: Popular Uses of History in American Life*. New York: Columbia University Press. 1998. Pp. x, 291. \$27.50.

This book by Roy Rosenzweig and David Thelen is extraordinarily important for all historians: those who work in the academy, in public venues, or in elementary and secondary classrooms. The book underscores what many in our profession have suspected for a long time: people who say that they do not like history are not dismissing history from their lives but rather saying that they are put off by classroom and lecture hall history because it appears so very disconnected from their personal experience. Recently, I gave a talk on community and history at a local bookstore. A young woman raised her hand and observed that the past was not useful to her; she lived in the present and saw little value in searching the past for clues to the present. Because she was accompanied by an older woman, I asked her how she had formed a friendship with her companion, suggesting that the relationship was constructed by mutual revelation of life stories, personal history. Yes, she responded, the woman next to her was her mother, and of course her mother had told her many stories about her family, her youth, and long-gone relatives. And yes, these stories revealed who she was, established personal identity, and defined her place in the world around her. This obvious loss of connection between what the young woman defined as history and her own intimate use of the past is exactly the point quantified in this ground-breaking book through interviews with a diverse group of informants and analysis by the authors.

The crucial conclusion for all of us in historical professions is that Americans actively pursue and use history, as abundantly indicated in the carefully collected data examined in this book. Americans look at photographs to remember and converse about personal pasts and take photos to preserve memories; they watch movies and read books about the past, visit museums and historic sites, attend reunions, investigate family history, keep journals and diaries, and participate in group activities that study, preserve, or present the past. They do these things in astounding numbers. Eighty-three percent, for example, take photos to preserve memories.

Even more revealing, however, are the responses to what activities make people feel most connected to the past and what sources people consider to be most reliable. Given six choices on what most connected them to the past, informants ranked family gatherings first, visiting museums and historic sites second, and

then celebrating holidays, reading books about the past, watching movies or television programs about the past, and studying history in school. When asked what source of information about the past they regarded as most credible, respondents ranked them in this order: museums, personal accounts from grandparents or other relatives, conversations with those who participated in or witnessed the events they described, college history professors, high school teachers, nonfiction books, and, finally, movies and television.

Rosenzweig and Thelen describe how informants related past to present, concluding that the past exerts powerful influences in the present and then explaining how people use the past to frame and pursue future possibilities. This quantification of what seems obvious furnishes a legitimacy that traditions and stories can no longer supply to a world that equates methodology with truth. Subsequent chapters relate awareness of the importance of the past to age, experience, and survival in the present and future. The work confirms that, even for individuals, the meanings of the past change in light of subsequent experiences. People interviewed wanted to get as close to the past as possible, and hence the credibility of historical sources lessened as the degree of mediation by teachers, professors, writers, and movie makers increased. The authors found that most informants connected their personal pasts to larger narratives but that white Americans connected to national narratives differently than people who identified with other racial and ethnic groups. Predictably, people with American Indian and African ancestry distrusted mediated history in schools even more than white Americans did.

The book concludes with two essays, one by each author. Rosenzweig and Thelen both ponder how professional historians can surmount the barriers between professional and popular history making. Yet their book seems to have discovered one more avenue for historical inquiry, instead of interpreting the findings as a clarion call for radical change in professional history. This is my sole frustration with the argument. The authors are no doubt aware that this kind of change requires us history professionals to relinquish exclusive authority over the significance and meanings assigned to the past—a disorienting but necessary experience. Many public history organizations have already attempted the experience, working with communities to diminish the boundaries between popular and professional uses of history and thus becoming models for change, models for all of us.

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DAVID HAMER. *History in Urban Places: The Historic Districts of the United States*. (Urban Life and Urban Landscape Series.) Columbus: Ohio State University Press. 1998. Pp. xiii, 277. Cloth \$49.95, paper \$19.95.

The first book on a proliferating American approach to interpreting and using history should be required

reading for anyone concerned with the “history” in historic districts. These districts are the most common and most effective prescription of preservationists for saving and interpreting the past.

Criticism, as David Hamer explains, centers on concerns that historic districts often celebrate only a particular, isolated interpretation of the past that suits contemporary needs. Discovering an area’s actual history is often less important to district makers than creating a tool to stop urban decay, to bolster sagging morale or real estate values, or to create a historical theme park to attract commerce.

District designation processes often overlook long periods of decline and invasion of ethnic minorities. Instead they glory in a past golden age when the community’s movers and shakers lived there. Districts also represent. Hamer adds, “a decline in optimism about the future, a turning away from the linear model of faith in perpetual progress to nostalgic invocation of the past” (p. 26). “My principal aim,” Hamer writes, is “an assessment of which facets of American urban development are represented among historic districts and which are not” (p. ix). Historic districts, for instance, rarely reflect the transient populations and transient architecture and usage that typify American cities. Furthermore, “the history that is being remembered is not the history of the ancestors of the people who now live in them” (p. 97).

Hamer bases this book not only on the published history of the preservation movement but also on a plethora of planning publications, including individual historic district documents. He examines a tremendous number and variety of historic districts throughout the U.S. and explores the historic district theory of geographers, historians, sociologists, and planners, ranging from Herbert Gans to John Stillgoe, from Jane Jacobs to Kevin Lynch.

Historic districts are traced to the 1920 creation of Charleston’s Society for the Preservation of Old Buildings. Spearheaded by a woman realtor, this organization procured a zoning ordinance designation for the “Old and Historic Charleston District” in 1931. Ironically, the short term goal was to prevent the Standard Oil Company from building a gas station in a historic area of the antebellum South Carolina city. That gas station, of course, might have helped to finance the Rockefellers’ reconstruction of Colonial Williamsburg, another prototype for many subsequent historic districts.

Although a few districts existed before the 1960s, it was widespread reaction to the Urban Renewal movement which generated thousands of private historic preservation societies and city-ordinance-created landmark commissions that typically create districts. The Federal Tax Reform Act of 1976 greatly expedited the process by making districts eligible for federal tax breaks and subsidizing the creation of state historic preservation offices that have sanctioned historical districts. This tax act allows owners of income-producing properties that were designated landmarks or are

contributing structures in designated historic districts to depreciate expenses for preserving income-producing property.

Hamer identifies and explores four stages of historic districtification: their original settlement and rise to prominence; subsequent history and transition, often involving urban decay; the history of historic preservation efforts leading to district designation; and the subsequent history of the area as a district.

Unlike many who bring a sense of passionate advocacy to the literature of historic preservation, Hamer is dispassionate and critical. He focuses on the problems rather than glorying in the success stories. He especially addresses the problems of gentrification and notes that now black neighborhoods are typically designated for achievements of previous non-black residents. Hamer neglects Native American, Latino, and Asian-American historic neighborhoods and, as his title indicates, the many districts in rural areas or ghost towns. Nor does he say anything about historic districts in other countries. But Hamer does offer a good, far-ranging exploration of urban historic districts and fits them into the larger context of urban history and planning. This thoughtful, stimulating book discusses a wide variety of perspectives and alternatives, such as Nashville's "neighborhood conservation districts" and San Francisco's "architectural conservation districts."

An estimated 15,000 national and/or local districts in the U.S., Hamer concludes, "represent the desire of Americans to aspire to the ideal of 'community,' even if they are seldom able to achieve it" (p. 205).

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GILBERT M. JOSEPH, CATHERINE C. LEGRAND, and RICARDO D. SALVATORE, editors. *Close Encounters of Empire: Writing the Cultural History of U.S.-Latin American Relations*. Foreword by FERNANDO CORONIL. (American Encounters/Global Interactions.) Durham, N.C.: Duke University Press. 1998. Pp. xv. 575. Cloth \$59.95, paper \$19.95.

This collection of essays on U.S.-Latin American cultural encounters represents a major advance in the scholarly study of cultural relations. Although it has long been known that cultural transmission and reception are very different things, thus far most scholars in the field have concentrated on official cultural policy or analyzed perceptions on the sending end. This volume, by contrast, takes cultural studies to the periphery by studying on-the-spot interactions. As Gilbert M. Joseph notes in the lead essay, it seeks to make good "the absence of cultural analysis from the overseas history of U.S. expansionism and hegemony" (p. 6).

In addition to that laudable ambition, the book has theoretical aspirations. The ten "empirical studies" that form its core are sandwiched between three

opening essays that deal with "theoretical concerns" and three "final reflections." Given the book's title and the rationale for the series (in which it is the initial volume), the sensibility is intended to be critical. As Joseph is quick to point out, "our use of the word *encounter* . . . should not be construed as a euphemizing device, to defang historical analysis of imperialism" (p. 5).

Be that as it may, the empirical studies are by far the best thing about this book, which provides a refreshing change of pace from the world of international relations as seen from Washington's corridors of power. One expects uneven contributions in an anthology, but the level of quality of the contributions in this collection is uniformly high. The essays are stimulating not only for their variety and freshness but also for their open-minded willingness to deal with the complexity of intercultural encounters in ways that do not necessarily harmonize well with the critical, anti-imperialist tune announced at the outset.

To be sure, a few of the essays do echo the imperial theme. Ricardo D. Salvatore takes a look at the ways in which Americans represented Latin America through "natural history exhibits, world fairs, travel books, plays, congressional handbooks, statistical handbooks, lectures, etc." (p. 96). The practices he describes were akin to cultural lepidoptery: "Knowledge was the virtual territory of empire, the instrument for placing the southern continent under the gaze of the United States . . . the project of knowledge managed to put South America on permanent display" (p. 94). Eric Paul Roorda looks at "the cult of the airplane" in Rafael Trujillo's Dominican Republic and details the symbolic power of a "shop window" air force that "demonstrated to Haitians and Dominicans the technological superiority and power of surveillance possessed by the ruler of their countries, the United States" (p. 269).

But for most of the contributors, if it is indeed cultural imperialism that they are talking about, it is imperialism with a difference. Deborah Poole's "Landscape and the Imperial Subject" surveys and interprets with grace and subtlety U.S. images of the Andes. Each American sent to the Andes, she says, "carried with him (or, more rarely, her) aspirations and emotions that sometimes contravened the business of empire and the interests of capital" (p. 132). In her essay on life in a United Fruit enclave, Catherine C. LeGrand concludes that "many studies of capitalism have fetishized large foreign firms, incorrectly according them a kind of omnipotence or portraying them as the sole connection to the global economy" (p. 356). Eileen J. Findlay's "Love in the Tropics" is a fascinating look at how America's introduction of new laws legalizing divorce affected Puerto Rican women. The relative absence of coercion in U.S. policy, in combination with a good deal of selective appropriation and some liberating consequences for Puerto Rican women, leads her to conclude that there is little value in

applying Foucauldian notions of discourse to American imperialism.

Stephen C. Topik's "Mercenaries in the Theater of War" recounts a wacky episode in 1893 in which an American merchant put together a squadron of twelve ships to intervene in a civil war in Brazil. For Topik, this was less a display of filibustering power than an illustration of how "private U.S. interests could conduct foreign policy with only the occasional assistance or acquiescence of the U.S. government" (p. 174). In the end, however, the naval intervention, which was chiefly a symbolic and ceremonial display of power, was "buried under a layer of Brazilian nationalism and virile heroism" (p. 174). Michael J. Schroeder, in "The Sandino Rebellion Revisited," takes a closer look at the first Sandinistas in Nicaragua. While insisting on the imperialist character of the Somocista Guardia Nacional, he adds the important qualification that it was also "an integral part of Segovian (and Nicaraguan) society, deeply embedded in the social fabric of the region (and country)" (pp. 248-49).

And so with the remaining contributions. Stephen Palmer's essay looks at the Rockefeller Foundation's anti-hookworm program in Costa Rica between 1914-1921, which was "part of an imperial plan" that at the same time was used by Costa Rica to "expand the sway of the state and to extend Costa Rican nationalism among the populace" (p. 328). Thomas Miller Klubock's essay on life in a Chilean subsidiary of Kennecott Copper describes the transition from welfare capitalism to post-World War II free market policies, which, while harmful to worker security, "undermined the hegemonic hold of the ideal of the monogamous male-headed family" (p. 395). Seth Fein's survey of "Film Propaganda in Cold War Mexico" concludes that "the films interacted with national and local mentalities that often resisted or simply did not care about U.S. foreign policy, even as individuals approved of the exhibitions as sources of entertainment, information, or both" (p. 437). Lauren Derby's "Gringo Chicken With Worms" unfolds the cultural meanings of a boycott of foreign-owned fast-food chicken establishments in the Dominican Republic as an example of cultural resistance to culinary imperialism, something that any French person today can understand.

In short, "cultural imperialism" is sliced, diced, scrambled, and pureed so thoroughly by these essays that one wonders what is left of the idea. One wonders, too, from what standpoint those who insist upon its application can demonstrate its overriding utility, especially as the postmodern temper acknowledges that all perspectives are equally subject to deconstruction. For Joseph Schumpeter, imperialism was an atavism. For many scholars, it would appear that the *idea* of imperialism has become a conceptual survival.

Although the essays effectively undermine simplified notions of cultural imperialism, their worm's eye view of history also tends to isolate events from any broad historical framework. As Schroeder acknowl-

edges, "the closer to the ground level one gets . . . the more complicated, ambiguous, and contingent things become" (p. 247). One obvious alternative, globalization, is pretty much ignored because, one assumes, it conflicts with and may well contradict the volume's dyadic fixation on U.S.-Latin American relations and because it is difficult to reconcile with the atomistic bent of the essays. Ironically, the postmodern temper, which is so often identified with impenetrable theorizing and hostility to historical positivism, in these essays is surprisingly conducive to microscopic fact-grubbing history, albeit microhistory of a very sophisticated kind.

There is no room in this conceptual Lilliput for the Brobdinagian state and its foreign policy. If the state was an epiphenomenon in Marxism, it is virtually a non-phenomenon for postmodern historians for whom traditional policy history is the conceptual enemy. As if to compensate for this exclusion, the discussions verge perilously close at times to defining culture as power. There is some truth to that, of course, but only some. How international politics, power, the state, warfare, and colonialism (and anti-imperialism) fit together with what these essays describe in such fascinating detail is not clear. Given the absence of serious attention to the role of high policy and geopolitics as agents of globalization, the essays in this volume, interesting as they are, amount to the resourceful practice of a historical pointillism that has no larger point. Perhaps that is why imperialism as a *Grundkonzept*, despite serious questions about its capacity to generate accurate historical descriptions at either the micro or the macro levels, continues to be attractive. At least it provides a cosmological veil to dress up what otherwise would be a naked preoccupation with the historical equivalent of quantum mechanics.

The volume's theorists are at sixes and sevens in dealing with these larger issues. Steve Stern grapples with the "paradoxes" he finds at work and cautions against succumbing to "conceptual despair" (p. 51). He finds solace in the idea of "partial negation," in which one truth is apparently truer than another, a Ptolemaic formulation that seeks to preserve the global framework at the center of his conceptual universe. By contrast, Emily Rosenberg is content to avoid the "universalist, objectivist master narratives of modernism" and settles for "more modest goals: to illuminate through partial glimpses, to attend to localized context, to deal sensitively with multiple stories and protean symbolic systems" (p. 510). Joseph, whose notes provide a valuable bibliographical introduction to the literature of the field, wants to have it both ways but provides no legible roadmap for negotiating the "major challenges" that he introduces and abandons at the end of his essay.

In short, this volume raises at least as many questions as it answers. Nevertheless, its essays make for interesting and stimulating reading in their own right and, as a bonus, raise important issues of conceptual-

ization and methodology. It deserves a wide readership.

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DONALD F. STEVENS, editor. *Based on a True Story: Latin American History at the Movies*. (Latin American Silhouettes: Studies in History and Culture.) Wilmington, Del.: Scholarly Resources. 1997. Pp. xii, 243. \$50.00.

This volume highlights a somewhat unpleasant fact of life for history teachers. Most Americans learn their "history" from movies and television, yet the accuracy of those fictionalized dramas is notably flawed. Holes in the narrative, questionable representations of events and personages, and outright falsehoods all characterize even the most faithful of efforts to dramatize the past. Although Hollywood has in recent years gone to extraordinary lengths to include renowned historians as advisors on the sets and screenplays of films such as *Amistad* (1998) and *Saving Private Ryan* (1999), for example, this effort seems only to multiply the howls of protest from other historians in the academic community. But are historically based dramas and teaching so diametrically opposed? Does introducing commercial cinema into the classroom really compromise the learning process? Like it or not, our students, along with the public at large, are exposed to and, dare we admit it, learn from fictionalized depictions of the past. Rather than buck the tide, this book offers a useful and engaging tool for history teachers in navigating one's way through this dilemma.

Editor Donald F. Stevens opens this diverse collection of essays with a rather provocative proposition. Historians, he suggests, are not so fundamentally different from film directors. As recreators of the past, both inevitably—indeed, necessarily—use poetic license to weave a believable narrative for their respective audiences. "Do we not do the same thing in our own minds (that is, visualize an imagined past based on the evidence) when we read traditional historical documents or narratives?" he asks (p. 6). Both written texts and film are "based on a true story," for both strive "to make the past presentable." Indeed "for all their apparent differences, [they] are rather similar," Stevens concludes (p. 10). Perhaps for that reason alone, films might claim a place in the classroom. But, more importantly, students respond to films in a visceral way that even the best of historical writing struggles to elicit. Rather than dodge the popular appeal of films, thus, this volume offers an eclectic series of interpretations for harnessing film's magical power while setting the historical record straight in the process.

The eleven essays (plus introduction) in this collection engage a cross-section of historically based cinematic dramas dealing with significant themes in Latin American history. The chronological, thematic, and

stylistic range of the films included for discussion is impressive and makes the book especially useful in a variety of teaching contexts. Of the thirteen films discussed (two chapters deal with more than one film), only two are in fact Hollywood productions (*1492: Conquest of Paradise* [1992] and *The Mission* [1986]). In some ways, this is unfortunate, for there are many appropriate Hollywood films on Latin America that students would find accessible. Still, the emphasis on Latin American cinema (as well as Werner Herzog's *Aguirre: The Wrath of God* [1972]) is certainly appropriate, although it should be pointed out that the four countries whose film industries are strongest are represented to the exclusion of other Latin American cinema. These countries and the respective films discussed are: Argentina (*Camila* [1984], *Miss Mary* [1986], *The Official Story* [1986]), Brazil (*Gabriela* [1983], *Pixote: A Lei dos Mais Francos* [1981]), Cuba (*The Other Francisco* [1975], *The Last Supper* [1976], *Lucía* [1969]), and Mexico (*Like Water for Chocolate* [1992]).

The essays offer an informative and often provocative analysis of how films depict bygone eras while simultaneously reflecting (often in unintended ways) on the historical conditions under which they are produced. For example, authors Sonya Lipsett-Rivera and Sergio Rivera Ayala offer an excellent critique of Ridley Scott's depiction of Christopher Columbus in *1492*, which they argue reinforces the "familiar nineteenth-century ideological construction of Columbus as hero" so fundamental to "American ideology" (p. 15). The film, as the authors point out, was produced in the context of the quincentennial celebrations and a debate over Columbus's legacy; as Columbus's reputation was assailed elsewhere in Latin America, Hollywood stuck to the narrative formula with which U.S. audiences were most comfortable. In a completely different context, we see a similar process at work with the Cuban film *Lucía*, whose approach and subject matter—a neo-realist, allegorical chronicling of three women's lives (all named *Lucía*) across the history of modern Cuba—were directly related to the nation-building project of the Cuban revolution (p. 126). One of the strengths of this book, in other words, is that the essays demonstrate to students the importance of "reading" films critically, much as students would other kinds of texts. This lesson applies not only to uncovering factual inaccuracies but also to an understanding of how cinema uses other devices such as lighting, framing, and soundtrack to convey an argument.

Although virtually all of the authors address this question of textual analysis, some succeed at pushing the discussion further and in more interesting ways than others. This is especially true of John Mraz (who as an historian writes on film and has directed two award-winning documentaries on Latin America) and of the essay co-authored by Rivera Ayala (whose background is in literature). The other contributors are historians first and film theorists sometimes a

distant second. This is not necessarily a disadvantage, as the book's primary audience is undoubtedly undergraduates in courses on Latin American history rather than cultural/film studies. Correcting inconsistencies between a cinematic representation of history and the historical record is a job best left done to historians. It does lend a certain uneven quality to the essays, however. Some authors stick to a more or less straight exegesis of the plot structure, while others take greater analytical risks of interpretation.

With one exception, all of the essays seem to concur on a central point: that cinema can be a useful tool in the classroom no matter how egregious the historical flaws. The one exception to this consensus is an essay by James Saeger, whose discussion of *The Mission* was first published in *The Americas* (January 1995); evidently, it is the only essay originally written outside of the context of this volume. Whereas the other authors regard their films as rich, if often problematic texts, Saeger views the relationship as more inherently antagonistic. Thus, after destroying much of the historical accuracy of *The Mission* (which students generally love), Saeger grimly concludes: "Future historians must correct [the filmmakers'] errors" (p. 83). The other authors, however, tend to view this relationship not only as a battle to correct the "fictions" of film but moreover as an opportunity for inspiring students to think about historical representation more generally.

This is a book that has been and will continue to be welcomed by teachers of courses in Latin American history and society. For those of us who incorporate film into our classes, there is always a concern about the costs of misrepresentation. With this collection, we can continue to assign these films knowing that a "corrective" and thought-provoking companion text can be used alongside. Part history text, part film analysis, these essays individually and collectively serve as an excellent supplemental reading for introductory classes. Another positive feature in this regard is the "suggested readings" list that follows each chapter, which might be especially useful for high school and world history teachers less familiar with the specific literature of the cases discussed.

My one gripe with this collection concerns the selection of films reviewed. Any collection of this nature will inevitably face criticism along these lines, but it strikes me as odd that such Hollywood classics as *Under Fire* (1983) (on the fall of Anastasio Somoza in Nicaragua), *Salvador* (1986) or *Romero* (1989) (on the civil war in El Salvador), and *Missing* (1982) (on the overthrow of Salvador Allende in Chile), just to mention a few, are not included, especially since they address thematic and geographical areas that the book leaves out; students also are apt to find these films particularly accessible. Moreover, *The Other Francisco*, *Lucía*, and *Gabriela* are all difficult to locate on video, which makes incorporating these essays into the classroom somewhat problematic. But these are relatively minor complaints that can be addressed in an expanded future addition. The strongest aspect of this

book is that it serves as a model. Students enjoy watching movies. This book encourages them to do so, unraveling (and rectifying) some of the embedded historical complexities while exposing the dramatic effects for what they are: sheer entertainment.

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MICHEL LE GALL and KENNETH PERKINS, editors. *The Maghrib in Question: Essays in History and Historiography*. Austin: University of Texas Press. 1997. Pp. xxv, 258. \$40.00.

Historians of the Maghrib—the North African countries of Morocco, Algeria, Tunisia, and Libya—have since the 1960s been struggling to establish a distinct identity for their field within American universities. This volume, edited by Michel Le Gall and Kenneth Perkins, is a collaborative effort by both American and North African scholars to assess the progress made toward this goal.

Introductory essays, one by a pioneer in the field, Leon Carl Brown, the other by veteran scholar Wilfrid Rollman, pose a crucial question. What should be the "frame of reference" for the history of the Maghrib? The Maghrib is, for academics, a sort of loose puzzle piece that might be made a part of at least four larger regions: Mediterranean, Islamic, Arab, or African. But this search for the wider frame of reference seems to run at cross-purposes with another dominant item on the Maghrib history agenda since the 1950s, that of "decolonizing" history and building the identity of the now independent nations that compose the region. Most of the essays in this volume demonstrate that the limited frameworks of the Maghrib and its component national states continue to prevail in scholarship. Rollman and several other contributors express discontent with this state of affairs, but they find no simple formula to remedy the problem.

Some contributions point toward the possible reasons for and ramifications of the prevalence of the national frame of reference. In her survey of the last thirty years of study of the medieval Maghrib, Mounira Chapoutot-Remadi expresses regret at the lack of interchange among the academic centers in the different countries of the region. She observes that Maghrib historians tend to focus on those periods in their medieval history that most easily fit, in terms of cultural style and territorial scope, in to the vision of a national development process. A by-product of this tendency is that Maghrib historians pay little attention to Muslim Spain in spite of the myriad cultural, social, and political links that span the Straits of Gibraltar.

Several of the essays survey phases in the development of national histories from traditional chronicles to nationalist identity building to post-independence efforts to incorporate new methods, especially those of the *Annales* school. Omar Carlier reflects on rival secular nationalist and Islamic trends in Algerian history. He argues that the Algerian government,

through its funding of education, research, and scholarly publications, established itself as the arbiter between these rivals. It might be argued, however, that until the rival camps negotiate a synthesis that accommodates all streams of the Algerian identity, this state of affairs is likely to prevail.

Le Gall's essay on Libya raises another dimension of the question. In Libya, as elsewhere in the Maghrib, large amounts of archival material remain inaccessible. Le Gall emphasizes the political element in this, the Qaddafi regime's fear that the archives might hold material that undercuts its own reading of history. Among Muammar al-Qaddafi's concerns are the rival source of legitimacy connected with the Sanusi monarchy overthrown in 1969 and the problems raised for Libya's image in Sub-Saharan Africa by the slave trade. The question of access to recent, postcolonial archival material in the Maghrib is not raised in this volume, and one wonders if serious research on postcolonial history will ever be possible unless there is a dramatic change in the political culture of the region.

Some contributors do work within larger frameworks. Julia Clancy-Smith's study of socially marginal groups and smuggling works within a Mediterranean frame of reference. She pays special attention to a group that evades classification as anything but Mediterranean: the Maltese, whose language is a blend of Italian and Arabic. Yet in making the Mediterranean a framework for the study of disreputable social roles and illegal activities, Clancy-Smith in a backhanded way reinforces the dominance of the national paradigm as the source of all that is historically praiseworthy and serious.

Ronald Messier reviews scholarship on the early centuries of Islamic North African history, a period in which the development of trans-Saharan trade links and the spread of Islam south of the Sahara have received considerable attention from scholars, who have been obliged to rely on scanty written sources, many of them second hand. Messier brings to bear on the subject new archeological tools, looking at Sijilmasa, a center for the trans-Saharan trade. It is an example of how new technology can sometimes do more to illuminate a remote and poorly documented past than it can contemporary history. One can not imagine an American scholar being invited, with all the appropriate high-tech equipment, into the archives at Tripoli.

A theme raised at many points is the way in which Maghribi history since the 1960s has been shaped by a reaction against French colonial historians, many of whom devoted themselves to justifying colonial rule by demonstrating the backwardness of the Maghrib's people and their lack of aptitude for self-government. The archvillain in this enterprise was the University of Algiers geographer Émile Félix Gautier, who interpreted the Maghribi past through a kind of environmental determinism. As a result, geographical and environmental studies have acquired a negative reputation since the end of colonial rule. This is unfortu-

nate, for it makes it difficult to resolve the key problem demonstrated by this book: how to escape the limitations of the national frame of reference and win a wider audience for Maghrib history.

If there is anything about the Maghrib that catches the attention of the outsider, it is certainly the environment, a key feature of which is vast stretches of arid land. This invites comparison with other arid lands, which might share aspects of culture with the Maghrib (such as Muslim Central Asia), or might not, as is the case with the American West. There is one book mentioned, Wil D. Swearingen's *Moroccan Mirages* (1987), which offers a new strategy for environmental history in North Africa that might pass muster with the decolonizers. Swearingen argues that French agricultural experts made serious errors by exaggerating similarities between Morocco and California, and further that French nationalist economic policies blunted whatever success they achieved in agricultural development in North Africa. But most importantly, Swearingen's work demonstrates the great potential of comparative study for opening new horizons in Maghrib history.

It is not accidental that Maghrib historians often gravitate to world history, for the Maghrib has been and can be a crossroads between very different cultural regions. It has had intensive relations with all of these regions and its history and culture cannot be adequately understood without examining all aspects of this larger context. The solution for the dilemma of how to fit the Maghrib into a larger frame of reference would seem to lie not in a simple revolutionary step, such as the "decolonization" movement of the postindependence years, but rather in advances on several different fronts, each potentially sensitive in political terms when viewed from the nationalist perspective. One of these fronts is the study of Maghribi communities removed from the region. A second is the study of the Islamic dimension of the Maghrib, not simply as an element of national affirmation but as a factor in internal debates and an element in external dialogue. This would involve, for instance, examining the historical process that led to the efforts by the lay Catholic community of Sant' Egidio to promote a resolution to the Algerian conflict in 1995.

On a third front, Maghrib history needs to advance southward through the Sahara and its borderlands. Such an enterprise should involve encouraging research such as that of Messier on Sijilmasa to create a new, comprehensive view of the socioeconomic and environmental history of the Sahara. And it should venture into comparative study of the adaptation of Islam to different political and cultural environments, the history of communities of Sub-Saharan origin in the Maghrib, and the role of gender in cross-cultural relations.

These projects require building relations between Maghrib historians and those working in adjacent fields such as the Mediterranean, Europe, the Middle East, West Africa, and the Sudan. This, in turn, will

depend on envisioning the Maghrib not just as a distinct territory with its own traditions and identities but as a frontier zone where Europe, the Middle East, and Africa intersect. If Maghrib historians seriously put the Maghrib "in question," they may pioneer an important tradition in twenty-first century historiography.

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COMPARATIVE/WORLD

CONSTANCE CLASSEN. *The Color of Angels: Cosmology, Gender and the Aesthetic Imagination*. New York: Routledge. 1998. Pp. 234. \$21.00.

The striking title of Constance Classen's slim but wide-ranging volume is curiously inappropriate for the prescriptive element of her work. Sight and hearing, as she repeatedly points out, have always been the privileged senses, whereas touch, taste, and smell have been considered baser. Consistent with the dominant values of Western culture, sight and hearing are masculine; the others are feminine in their associations and attributes. Color, however, is mediated by sight, and since one of Classen's stated aims is to encourage enhanced appreciation of these "baser" senses, *the smell of angels* would have been the catchier title.

When Samuel Johnson was accused by an acquaintance of smelling, he remarked, "Madam, I stink, you smell." It may be that angels do not smell (or, at least, stink); saints and nuns could, however, and in one of the volume's more memorable chapters, on the odor of sanctity, Classen recounts the tales of Lydwine of Schiedam, Teresa of Avilá, and several other sainted women whose holiness was expressed through a divine odor that they gave off, even after death. Saint Teresa, who had powerful olfactory sensations during her mystical raptures, gave off such a strong odor on her deathbed that she scented everything she touched. After she died, her body was disinterred, and the nuns given her sweet-smelling left hand as a souvenir. The hand kept impregnating the cloths it was wrapped in, but the demand for relics of Teresa became so great that, three years after her death, priests stole her body and took it from Alba to Avilá. The nuns of Alba were disconsolate, though they cheered up a bit when the kind monks cut off her arm (minus its hand) and left it behind to scent the convent.

These and similar medieval and early modern cosmological preoccupations with the senses occupy the first third of Classen's monograph. Section two, on gender, looks at the sensory attributes of witches and sensory imagery in the writings of Catherine des Roche, Margaret Cavendish, and other early modern women who dared to express themselves with their pens. Classen also has some shrewd things to say on the development of the sensory stereotypes of the two sexes and how the traditional hierarchy of the senses reinforced the sexual division of labor.

The final section turns to modern aesthetics, especially the attempts of symbolist, futurist, and surrealist artists to develop art forms in which the eye is not so privileged. Classen also invokes the moving testimony of Laura Bridgman, Helen Keller, and other deaf and blind individuals about what it is like to live without these two "noble" senses. How can we decide what is beautiful if we cannot see it or hear it? Modern experiments with olfactory and tactile art, and a description of a Tokyo museum for the blind, round out Classen's selective excursion through a millennium of the senses.

Classen's achievement is impressive, although the volume is so discursive that it remains suggestive rather than compelling. She is certainly correct that modern television-based culture is hypervisual and admirable in the way she argues that all fully sensed persons should be encouraged to cultivate the whole range of their senses. Classen is less convincing about tactile or olfactory aesthetics, even if such experiments deserve support. For most people, I suspect, things like the touch of a loved one, or the smell of fresh-brewed coffee, will still be differentiated from a trip to an art gallery. They will also probably be more highly valued.

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SANDER GILMAN. *Creating Beauty to Cure the Soul: Race and Psychology in the Shaping of Aesthetic Surgery*. Durham, N.C.: Duke University Press. 1998. Pp. xii, 179. \$21.95.

Sander Gilman's book ostensibly examines the relationships between aesthetic and reconstructive surgery, on the one hand, and psychiatry (primarily psychoanalysis) on the other. The book is, however, an impressively wide-ranging (despite its comparatively short length of 146 pages of text) excavation of the cultural interconnections between beauty, health, race, gender, and illness. Following the development of aesthetic surgery and the psychological theories for and against it, Gilman makes a convincing case for the ways in which stereotypes of race and gender have infused our understanding of health, beauty, ugliness, and disease.

Crucial to Gilman's argument is that cosmetic surgery is as much an intervention on the mind (at least according to many of its proponents and critics) as it is on the body. "Central to the project of aesthetic surgery," Gilman writes, "is the assumption that all physical changes are changes to the psyche and that the restoration of the Platonic ideal of beauty and happiness (through the invisibility of the subject) is possible" (p. 29). A further series of relationships frames Gilman's narrative. First, difference (perceived ugliness as shaped by stereotypes of race or gender) leads to unhappiness. Second, unhappiness is identical to sickness. Third, illness requires cure (such as surgical intervention). And, finally, beauty is health. In

short, aesthetic surgery rests upon a "somatopsychic" view, which is an inversion of the traditional psychoanalytic psychosomatic model: "For the psychoanalyst, psychic 'misery' is written on the body as physical symptoms; for the aesthetic surgeon, the 'unhappiness' of the patient is the result of the physical nature of the body" (p. 13). Both models presume a malleable and interconnected mind and body.

Although the promise of personal transformation flowed from the Enlightenment, it was not until the nineteenth century that medical developments (notably anesthesia and later asepsis) permitted surgeons to attempt significant aesthetic interventions upon the body. In the 1830s, for example, surgeons developed techniques to hide nasal deformities caused by syphilis. The aim of these operations was both psychological (to heal the sufferer's distress at being marked with sin) and social (to efface the difference between the individual and those not afflicted with disease as marked by their "normal" noses). This basic logic holds throughout the history of modern aesthetic surgery: "(In)visibility is the goal of all aesthetic procedures. Aesthetic procedures are intended to move an individual from being visible in one cohort to being a member of another cohort or collective, which is so visible that its visibility becomes defined as the 'normal'" (p. 4).

Beginning at the end of the nineteenth century, race, especially in the guise of the Jewish nose, replaced disease as that which was meant to be hidden through aesthetic surgery. Gilman shows the ways in which science legitimated the Jewish nose as a signifier of Jewish character and how these prejudices became linked to the goals of aesthetic surgery. Since World War II, Gilman argues, the social meaning of "ugly" has shifted. Increasingly, race has been replaced by gender.

Since cosmetic surgeons have seen their work as therapeutic, it is not surprising that they often justified their work on psychological grounds. Indeed, psychological theory played an important role in underpinning and, at times, undermining the notion of cosmetically altering the body as a form of psychotherapy. Before his abandonment of the seduction theory, Freud's work lent support to the idea that surgeons were indeed doing more than simply acquiescing to their patients' vanity. This was especially apparent in Freud and Wilhelm Fliess's theories that connected mind and body. Freud's later work however, proved less hospitable to the kind of simple psychological theorizing that justified cosmetic surgery as a form of psychotherapy. Freud's abandonment of the trauma theory and its replacement with unconscious conflict and fantasy made the body, at best, a poor site for intervening as a means of treating a patient's unhappiness. That is not to say that psychoanalytic theory did not continue to bolster the claims made by aesthetic surgeons. For instance, a simplified version of Alfred Adler's inferiority complex was readily incorporated into the psychological justification of aesthetic surgery wherein a person's sense of inferiority could be ex-

pressed in dissatisfaction over a particular body part. Surgical intervention, then, could be one means by which to treat psychological distress. Gilman also examines as well as utilizes other theorists, such as Paul Schilder and Karl Menninger, in this cultural history of cosmetic surgery.

Psychoanalysis now has scant influence on how plastic surgeons construct their psychological theories of the efficacy of their work. I wonder, though, about the extent to which contemporary psychiatry, with its vaunted potential for "cosmetic psychopharmacology" (a term coined by psychiatrist Peter Kramer, author of *Listening to Prozac* [1993]), has influenced and has been influenced by aesthetic surgery. As Gilman's highly nuanced work suggests, the answer to this question is laden with cultural meanings, which may entail a number of unfortunate racial and gendered stereotypes.

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TERENCE C. HALLIDAY and LUCIEN KARPIK, editors.
Lawyers and the Rise of Western Political Liberalism: Europe and North America from the Eighteenth to Twentieth Centuries. (Oxford Socio-Legal Studies.) New York: Clarendon Press, Oxford University. 1997. Pp. 379. \$28.00.

Since the 1980s, totalitarian and authoritarian regimes have yielded before the pressures of global democratization and political liberalism. Why have these changes come about? Scholars in the new academic discipline of "transitional studies" usually point to economic pressures, notably the emerging global marketplace. This book, however, offers a different explanation. Editors Terence C. Halliday and Lucien Karpik, both sociologists of law, insist that lawyers are among the most, if not the most, potent agents of global liberalism. They make their case through sophisticated historical essays about the rise of modern legal professionalism in England, France, Germany, and the United States. According to Halliday and Karpik, market forces are a necessary but not a sufficient condition to develop a liberal political culture. Instead, they shift attention toward political life, civic affairs, and the place of lawyers in both. Politics, not economics, shaped the liberal state; lawyers emerged in the last century as the facilitators and mediators of that state.

Halliday and Karpik divide the book into nine chapters, with each of the four countries receiving attention in two chapters. An introductory chapter by the editors maps the problems addressed in the other essays; a postscript by them offers a general theoretical framework for further study. All of the essays have a strongly historical approach, assessing the ways in which lawyers promoted the rise of liberalism. Michael Grossberg, for example, examines the legal aid movement in America during the first third of the twentieth

century. Legal aid provided representation to persons who could not otherwise afford it. If the rule of law is vital to the rise of the liberal state, Grossberg argues, then it follows that everyone must have the opportunity to have counsel. Grossberg's analysis, however, departs substantially from previous interpretations that described the legal aid movement as a response to the pressures of industrial capitalism. Instead, through a shrewd analysis of bar politics, he persuasively argues that legal aid was a lawyer's reform rooted in the American legal profession's much neglected humanitarian ethos. In essence, some American lawyers promoted the development of the liberal state by advancing the cause of legal aid.

In an equally impressive essay, Kenneth Ledford turns to the late nineteenth and early twentieth-century German experience. The case of Germany is particularly arresting as an example of what happened when lawyers in the West could not or did not perform their mediating roles, as was true in the complicity of the German bar with National Socialism. Ledford asks how this supposed ally of liberal political ideas could succumb to the Nazis? The explanation, he maintains, is that the bar sowed the seeds of its own collaboration. The mere existence of lawyers does not mean that liberalism will flourish. In the German case, a fervent commitment to procedure and process swallowed up substantive justice. Moreover, as the early twentieth-century German bar became socially open, it also became more stratified and subject to internal conflicts. With the bar divided over the economic and political changes associated with National Socialism, it failed to fulfill one of its key responsibilities: representation of the public interest.

All of the essays are notable for the close attention they pay to internal bar politics. In their postscript, Halliday and Karpik provide an excellent comparative analysis of the ways in which differing national approaches to internal bar issues, such as organizing and disciplining the legal profession, shaped the impact of law on the political culture. In the West, two rival models emerged. The English offered a strongly apolitical scheme that embedded the legal profession in the deep structure of the English constitution. The French model, by contrast, provided a constitutional status for lawyers that was constantly subject to changing political regimes and ideologies. The pressures of globalization, however, have prompted movement toward a hybrid American scheme that has encroached "on the English and the French as it has gathered momentum since the Second World War" (p. 363).

This book does exactly what it promises and does it well. It appropriately challenges the pervasively market-oriented explanation of the basis of liberal government, it makes a compelling case for using historical examples to advance dynamic models of legal and political interaction, and it affirms the close connection between a nation's legal culture and its capacity to engage in liberal politics. Indeed, the book is so provocative that a reader is likely to wish for more,

especially comparisons with the role of lawyers in non-Western nations, even to the point of asking whether lawyers are necessary at all. A non-Western example or two would have substantially refined Halliday and Karpik's arguments about the global consequences of the Western model they so elegantly detail. In this regard, the great wealth of transitional studies of politics, even with their sometimes too eager claims about the influence of the market, reminds us of the difficulty of applying Western models in an effort to fashion liberal regimes from formerly authoritarian circumstances. The political and legal chaos of modern Russia offers one reminder. In a somewhat more successful vein, so does the South African Constitution. It embodies German civil law principles, advocates human rights on a transnational basis, and relies on a legal profession that appears to be English, acts American, and borrows its arguments from around the globe. The new South African Constitution, however, deemphasizes the role of lawyers, giving citizens the opportunity to make their own case before a court, tribunal, or forum.

In sum, the historical examples mustered by Halliday and Karpik skillfully limn the development of the West's legal profession, but they are, perhaps, less persuasive in prescribing how the contemporary developing world might fashion liberal states from the ashes of authoritarian regimes. On matters of substantive justice, equality, and freedom, the West may have as much to learn from the history of legal change and the rise of liberalism in the developing world as it has to offer to that world.

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DANIELA SPENSER. *The Impossible Triangle: Mexico, Soviet Russia, and the United States in the 1920s*. Foreword by FRIEDRICH KATZ. (American Encounters/Global Interactions.) Durham, N.C.: Duke University Press, 1999. Pp. xiv, 254. Cloth \$49.95, paper \$17.95.

Much has been written about Mexico's relations with the United States during the 1920s. Similarly, a substantial number of studies have appeared in the former Soviet Union about Mexico's relations with the USSR. No scholarly work up to this point has attempted to explain in any depth the sometimes convoluted trilateral relationship that existed among the three countries during that decade, however. It is a subject that has long needed serious attention by scholars, and it is one that finally has been addressed by Daniela Spenser in this book.

Spenser begins by examining the ways in which the Mexican and Russian revolutions viewed each other and considers how the United States attempted to understand these two new forces that would play such a role in determining the history of the twentieth century. She provides useful summaries of the concerns and expectations of each of the protagonists before moving on to assess the more intensive inter-

action that followed the opening of diplomatic relations between Mexico and the U.S. as well as Mexico and the USSR. For Mexico, relations with the two countries were rocky from the beginning, and Spenser recounts with care the details of the ensuing problems. By the end of the decade, of course, it was clear in Mexico City that stable and proper relations with the United States were essential to Mexico's survival and that the Communist International's anti-Mexican policies made it impossible for official relations between Mexico and the USSR to continue. Spenser examines each turning point in the evolution of the relationships and provides convincing evidence to explain why developments took place as they did.

One of the important features of this study is the fact that to a great extent, Spenser's research could not have been carried out before the last decade. While she has used all the appropriate published materials for the book, she has also worked extensively in archives in Mexico, the United States, and Russia. Many of the Russian sources she consulted were made available only recently, and had she needed to complete this study on the basis of Soviet published materials, her book would have been as superficial as the pre-glasnost works of Soviet scholars that still dominate the bibliography in this area. Spenser has been able to transcend these limitations, however, and her sophisticated and judicious use of archival materials in Mexico City, Moscow, and Washington, D.C. have helped her produce a book that breaks new ground.

What is missing, however, is a sense of the liveliness of the times and the colorfulness of the individuals involved in the story that Spenser otherwise tells so well. The reader does not come away from the text with a feeling for the absurdities and contradictions so often present in the personalities of the diplomats, politicians, and revolutionaries who were charged with maintaining relations among the three countries, and who played such an important role in determining the direction those relations were to take. That being said, Spenser should be commended for adopting an appropriately professional tone in writing about a subject that has not always been addressed seriously. Her style is straightforward, clear, and refreshingly lacking in the wordiness and obscurity so typical of much academic prose today. Spenser is able to write for an academic audience, but her book should also appeal to a broader group of readers interested in the topic in Mexico and Russia as well as in the United States.

In sum, this is a fine book that fills a gap in our understanding of the diplomatic history of Mexico, the USSR, and the United States during an important decade. It is rigorously researched and carefully written. Its interpretations are judicious and its conclusions thoughtful and reasonable. It is, in the end, a valuable book that provides a new perspective on

important questions of interest to a range of scholars and general readers alike.

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NICHOLAS TARLING, *Britain, Southeast Asia and the Onset of the Cold War, 1945–1950*. New York: Cambridge University Press. 1998. Pp. x, 488. \$64.95.

The complex relationship between empire and Cold War, which this book attempts to address, remains a vital and relatively unexplored area for imperial and international historians. Within this context, Nicholas Tarling aims to place British policy toward the countries of Southeast Asia in juxtaposition with one another while linking them to the broader international setting. The book succeeds in the first aim but fails in the second.

Thoroughly researched and dealing with Burma, Thailand, Malaya, Indochina, the Philippines, and Indonesia, this is a detailed, blow-by-blow account of British policy and British attitudes toward the policies of its partners in the Western alliance. Minute by official minute, every aspect of policy and every official's view of policy and of the character of the protagonists is dutifully recorded. On occasion, the detail becomes overwhelming, and one longs for a glimpse not just of the broader international context but of a more general interpretative framework. Having said that, the portrayal of particular stages and shifts in British policy is often accompanied by perceptive analysis. Thus, the main value of the book is its provision of a lucid explanation of British policy within a regional setting. The concerns about how Dutch and French policy in Indonesia or Indochina affected British or former British territories add a valuable new dimension to the study of Britain's efforts to retain influence in Southeast Asia while wrestling with the problems of transferring power.

Dutch and French policies are not approached through the archives in France or the Netherlands but through the eyes of British officials and ministers. Purists may take issue with this, but British views of the French actions in Indochina provide a fascinating and informative window on the developing conflict there. It is a window that most definitely serves specialists on the relations between the European powers and their colonial subjects rather than generalists with an overall interest in the international importance of Southeast Asia after the end of World War II.

The attempts to preserve control and influence and the process of transferring power are really what this book is about. The focus is almost exclusively on the interactions between Europeans and their colonial subjects seeking independence, not on what the Cold War was about and how it was fought in Southeast Asia. Those primarily interested in the Cold War may be disappointed to find that it is not given the same degree of attention it receives in Robert J. McMahon's

book on South Asia, *The Cold War on the Periphery: The U.S., India, and Pakistan* (1994).

The Cold War enters the picture only as very general background seen through the most orthodox of lenses. The vital importance of European security and the alleged threat of Soviet power are thus central to Tarling's analysis of the Cold War conflict. Britain allegedly subordinated imperial and post-imperial concerns in Southeast Asia to its need to ally with the United States in the more vital European theater. This interpretation neglects the increasing importance—in British eyes—of what was known as the Far East before the outbreak of the Korean War. In part, this reflects Tarling's lack of interest in a broader imperial strategy designed to maximize British influence in international affairs while confronting the Soviet Union and the challenge of left-wing movements, as embodied in efforts from 1945 to 1949 to secure independence from and equality with the United States. It also stems from the relative lack of consultation of military, intelligence, and treasury sources. The former reveal the development of a Cold War fighting strategy that, in the global strategy paper of 1952, was given preference over both the deterrent and the preparation for hot war and that, in regional terms, identified East and Southeast Asia as the most significant Cold War region.

Consequently, this book does not add to our understanding of how the Cold War was perceived by policy makers at the time. British policy is explained simply in terms of a search for peace, prosperity, and stability in Southeast Asia. The attempts to use India and the Commonwealth as surrogates in order to avoid charges of imperialism are incorporated into this analysis. British statesmanship in terms of cooperation with the peoples of Southeast Asia is praised, but a detailed analysis of the extent to which policy changes in Burma and Malaya were influenced by necessity, Cold War concerns, or a quest for regional influence is lacking.

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DEBORAH WELCH LARSON. *Anatomy of Mistrust: U.S.-Soviet Relations during the Cold War*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1997. Pp. xi, 329.

The Cold War, according to Deborah Welch Larson, was not inevitable. The realist argument that the East-West confrontation flowed inexorably from the vacuum of power created by World War II and the geopolitics of two competing superpowers simply does not hold water. Both alliance systems were economically and strategically self-sufficient, she argues; no meaningful territorial disputes separated the United States and the Soviet Union. One was a land power, the other a sea power. And yet, undeniably, there was a Cold War that preoccupied and terrified the world for nearly a half century. Why?

In her gracefully written and cogent analysis, Larson

argues that the answers are to be found in psychology and ideology. Quite simply, the two superpowers were not willing to do those things necessary to build mutual trust. The opportunity was there. Domestic politics acted as a significant restraint in neither polity. Both the American and Soviet peoples wanted to avoid war, particularly nuclear war, and for that reason consistently supported meaningful arms limitation agreements. The executives in both countries were free—politically, institutionally, strategically—to pursue peaceful coexistence. The Soviet Union was a totalitarian, militarist power in which the military played a relatively minor role. Accumulation of power in the American executive meant that a determined president could control or override press, public, and Congress to impose its will on Soviet-American relations. Through five case studies—the period from Joseph Stalin's death until 1955; the post-Sputnik peace offensive; the window of opportunity following the Cuban Missile Crisis; the Nixon-Brezhnev detente; and Mikhail Gorbachev's accession to power—Larson shows how the lack of trust (based on “predictability, credibility, and good intentions” [p. 17]) prevented significant arms control agreements. Although both sides were to blame, Larson credits the Soviets with a greater willingness to make unilateral concessions that left them vulnerable. Dwight D. Eisenhower's desire to balance the budget and establish his legacy as a peacemaker made him willing, but John Foster Dulles's rigidity and intransigence prevented any significant breakthroughs. John F. Kennedy's decisions to build up America's conventional and nuclear arsenals touched off a series of events that brought the world to the brink of atomic warfare. Richard M. Nixon and Leonid Brezhnev made huge strides toward detente, but they failed to limit MIRV warheads which endangered deterrence and escalated the arms race. And, finally, despite Gorbachev's “heroic efforts” (p. 191), Ronald Reagan's deep mistrust of the “Evil Empire” prevented the conclusion of an agreement that would have slashed both sides' strategic arsenals in return for limits on space-based defenses.

This is a well-crafted, tightly argued book. The role of mistrust in the Cold War is hardly one that has gone unconsidered, but never has it been so systematically and convincingly examined. Larson excels as a historian rather than a political scientist. The narratives of the five missed opportunities are fast-paced, well-researched, and insightful. Her observation that Reagan was “a visionary, a radical who wished to abolish the doctrine of ‘mutual assured destruction’ (MAD) which he regarded as immoral” (p. 196) and thus naively embraced the concept of the Strategic Defense Initiative does not fit into her paradigm but is certainly one of her most significant findings. Larson has produced a study that will be of interest and use to all students of the Cold War, whatever their discipline.

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ASIA

JONATHAN D. SPENCE. *The Chan's Great Continent: China in Western Minds*. New York: W. W. Norton. 1998. Pp. xviii, 279. \$27.50.

What is a "Chan"? Jonathan D. Spence has picked the word from a Hart Crane poem imagining how China appeared in Christopher Columbus's mind, and the "Chan" is in fact the Khan of Marco Polo's *Travels* (1298)—itself to some extent a product of imagination—upon which Christopher Columbus based his conceptions of China. The ambiguity is deliberate: Spence's new book deals with images of China in Western texts throughout the last eight centuries: the "collective projections" (as C. G. Jung would have it) as well as individual visions. Spence digs deep for the roots of the perennial Western admiration-mixed-with-fear syndrome in relation to China, tracing the ever-growing complexity of this from thirteenth-century travel reports to the present times. The result is a splendid book. Less scholarly than previous works by Spence on the intellectual interface between China and the West, it is easy and enjoyable to read yet full of insights. It is also vast in its scope, and the interpretive framework is quite open, with few attempts at categorization.

The source material for the book is the lives and works of around fifty individuals who either explored China or employed notions of China in their literary works. The foundation of Spence's ongoing work is the biography in all its forms and formats; here through compression he manages to present the life and times of his sources as well as their contributions to the Western image of China. The fact that the book remains a coherent whole is due to Spence's remarkable talents as a writer: the individuals examined keep resurfacing, as do some of the ideas and literary works they produced, weaving the text together at several levels while at the same time effectively demonstrating long-term continuities in the Western perception of things Chinese.

The sources selected by Spence are introduced as "sightings," hinting at something fleeting or intermittent as the explorers struggle to get their bearings. The term sightings also applies to aim-taking, and in earlier times it had the added meanings of cheating at dice as well as sighing. All these meanings coalesce in a rich and varied selection of sources. Some are obvious choices: the Venetian merchant and adventurer Marco Polo, the Jesuit missionary Matteo Ricci, the British ambassador George Macartney, the French writer André Malraux, and the American president Richard Nixon. Other voices, such as Jane Austen's, are quite surprising. Indeed, the scope of the book is so wide that the reader cannot always gauge the criteria for the inclusion of a source.

In the opening chapter, Spence examines the enigmatic case of Marco Polo, who probably never visited China but whose tales nevertheless became ingrained in the Western discourse on China. The chapter on the

Enlightenment philosophers is among the finest in the book, as is the chapter on the "French Exotic" of the late nineteenth century: according to Spence, it combined elements of grace, sensuality, cruelty, and melancholy. In the closing chapter, Spence offers a sensitive reading of Chinese motifs in the works of Franz Kafka, Jorge Luis Borges, and Italo Calvino. Other chapters are less convincing. Some cases appear peripheral, such as a discussion of Bertold Brecht's 1930 play, "The Measures Taken," which is set in China, in a chapter on "Radical Visions"; the choice is not an obvious one, since the Chinese setting of the play has little to do with the substance of Brecht's images of China (a better case might have been Brecht's interest in the Chinese theater, triggered by Mei Lanfang's 1935 performances in Moscow).

Despite Spence's careful attention to context, there is sometimes a sense of being lifted out of history in this book; the images of China multiply as admiration and fear permutate and transmute, but what of the real encounters and the power relationships they produced? Spence devotes little attention to the implicit discourse of power that is embodied in the evolving image of China. Illusions hurt, but the nature of the damage has been very different on the two sides of the equation.

Spence treats his sources lovingly and respectfully, and he advises his own readers to remember the often difficult circumstances surrounding the "sightings." The lasting imprint of the book on the reader's soul is reinforced by the humanity and compassion that have become ever-deepening currents in Spence's great oeuvre.

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ANGELA ZITO. *Of Body and Brush: Grand Sacrifice as Text/Performance in Eighteenth-Century China*. Chicago: University of Chicago Press. 1997. Pp. xviii, 311. Cloth \$45.00, paper \$17.95.

Angela Zito's Ph.D. dissertation has been cited more often than many books, and its publication has been widely anticipated. Now that it is here, the book exemplifies the achievements and problematics of the turn toward critical cultural studies of China that occurred in the early 1990s—a trend in which Zito herself was a key participant, as co-editor (with Tani Barlow) of *Body, Subject and Power in China* (1994) and as a member of the editorial collective of *positions: east asia cultures critiques*.

As is characteristic of the "New Wave" in China Studies, Zito draws on interdisciplinary feminist, post-modern, poststructuralist, and critical theories. She uses them in a self-reflexive way to reveal the logic behind the practices of Grand Sacrifice in eighteenth-century China, particularly under the Qianlong emperor (r. 1736–1799), while simultaneously critiquing the logic of the theories she is using. The Grand Sacrifices (to Heaven, Earth, the Imperial Ancestors,

and the Altar of Soil and Grain) were the most important imperial rituals in the annual ceremonial cycle. Zito argues that they mediated a contradiction that lay at the heart of Manchu rule over the Han scholar-officials who governed the populace. "Body" and "brush" symbolize two different legitimating principles of power, the first derived from the patrilineal line of descent, the role of the filial (*xiao*) son, and the performance of ritual sacrifice; the second derived from virtue, the wisdom of the sage, and the production of texts (*wen*). Through the production of texts and the performance of the rituals of filiality, the emperor's influence was to radiate throughout the empire, a metaphysical notion that Zito translates as "centering." As "a man among other men (and the perfection of yang masculinity), [the emperor] provided the model of how to model for others who would emulate him" (p. 6). The elegance of Zito's description is its interlinkage of ideologies of gender and empire. The weakness is that its attention to gender is not matched by an attention to ethnicity, so that the overall impression is of a Sinicized Manchu emperor attempting to embody Han Confucian ideals of filiality and masculinity. Did ethnicity not complicate the effectiveness of the emperor's "centering"?

The postmodern critique of existing theories of text and ritual has, among other things, attempted to move beyond Cartesian dualisms in which "representation" assumes an ultimate reality that is being represented, and "ritual" assumes a distinction between (ritual) action and (religious) thought. Zito finds that these dichotomies did not underlie Chinese conceptions of text and ritual (*li*), which were framed within a different, non-Cartesian metaphysics. Thus, Chinese theorists come off rather better than the Western theories that Zito uses to analyze them, producing an interesting—and intentional—hall-of-mirrors effect: in the end, the reader is unsure whether the Western analysis is illuminating the Chinese texts or vice versa. Is Zito reading the Chinese texts, or are they reading her?

Her self-critique on ritual is not as effective as that on textuality, indicating two problems with the new interdisciplinary approach: no one can be expected to have equal mastery of multiple disciplines (history, literature, anthropology/sociology); and maintaining an unwavering, critical eye on oneself while ranging across three disciplines is even more difficult. To get beyond the limitations of the Western category of ritual, she proposes conceiving of *li* not as ritual but as a "discourse" that produces the ways of being human (everything from etiquette to ritual) that organize the cosmos. Having shown the misfit between the Western concept of ritual and the Chinese concept of *li*, however, she seems to use the words interchangeably (sometimes translating *li* as "ritual action" or "rites"). Western ritual theory has tried to define ritual not only by what it is but also by what it is not. This apparently did not concern Chinese ritualists, but the use of the narrower Western category as a translation of the polymorphous Chinese one confuses this point. On

this and other points, Zito does not seem fully to carry out a reflexive critique of the category of ritual. For example, Zito's main source on ritual theory, Catherine Bell (*Ritual Theory, Ritual Practice* [1992]), argued that "The notion that ritual resolves a fundamental social contradiction can be seen as a type of myth legitimating the whole apparatus of ritual studies" (p. 37). Zito's analysis of Grand Sacrifice as mediating the Manchu-Han contradiction seems to contribute to this kind of legitimization of ritual studies (though it also makes much broader points), reproducing in this area the Eurocentric assumptions that she so successfully delegitimizes in others.

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ANNE WALTHALL. *The Weak Body of a Useless Woman: Matsuo Taseko and the Meiji Restoration*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1998. Pp. xvi, 412. Cloth \$45.00, paper \$17.00.

Anne Walthall's work has, from the beginning, been characterized by her interest in breaking new ground and her unwillingness to accept easy answers. In that sense, this book on Matsuo Taseko seems very much on a continuum with her earlier work, but its scope is larger. Here Walthall is ready to contest important aspects of the "settled" history of the Meiji Restoration, as well as Japanese interpretations of Taseko, a woman lionized by the Meiji establishment as a model of their particular version of "good wife, wise mother" and by later recreators of the Japanese past as an uncomplicated advocate of the "Emperor system." Walthall's work makes it clear that Taseko was anything but uncomplicated, and she certainly does not seem to fit the mid-Meiji version of "good wife, wise mother." She is, however, very interesting, and she clearly lived in interesting times.

Japanese specialists on this side of the Pacific will find in the book a serious, and quite intentional, challenge to what Walthall describes as "a male-centered genre of history writing that demands the public story of the public life . . . Taseko became famous because she came to know some of these [public] men. Hers is not a success story: it disrupts the sequence of events, confuses public and private acts, foregrounds ideology, and makes sense only in terms of her class and gender" (p. 3). "In the sense both of who has been written about and who has done the writing, at least within the English speaking world, the Meiji Restoration has constituted masculine turf. Let us now proceed to violate it" (p. 16).

Although her published poetry, diaries, and letters may constitute a fuller historical record than those of many lesser-known women of her age, important parts of Taseko's life went undocumented, even by local historians. To fill these gaps, Walthall read widely in local histories and family archives, as well as biographies of women who were her contemporaries, looking

for information that other historians might have “considered beneath [their] notice” (p. 14) in order to discover “private” contexts against which to read her “public” life. This effort, I am happy to report, rewards us doubly by providing, first, a credible and complicated picture of Taseko and, second, a fascinating account of the possibilities and barriers many village women faced in late Tokugawa Japan.

By the time Taseko joined the Hirata school of nativist studies at the age of fifty, she had already enjoyed a full life as a mother of seven children, a major contributor to the family economy (and perhaps to that of the village as well), a poet, and a woman who had traveled widely on pilgrimages far from her village. Read against this background, one that suggests a strong sense of identity and a range of choices not typically associated with late Tokugawa women, Taseko’s later political activities in support of nativist ideology and its anti-Tokugawa ideologues do not seem surprising.

Walthall’s telling of Taseko’s story does not suggest that her political involvement was the result of some karmic destiny or the “natural” denouement of her life’s experiences; her decision to travel to Kyoto in 1862, then the center of pro-emperor, anti-Tokugawa, anti-foreign sentiment, was a remarkable choice, even for her. Walthall adds another proviso: “Had Taseko not come from money, she would never have gone to Kyoto. No matter how quick her mind, she would have spent her life as so many farm women did, mired in illiteracy and struggling from before dawn till after dusk to keep her family alive” (p. 81). Taseko does not seem simply to have come from money, however; Walthall makes a convincing argument, based on an analysis of Matsuo family records and contextual evidence from the village economy, that Taseko’s contribution (through sericulture) to the family’s wealth was very significant.

This is one of the best books on late Tokugawa I have read; it demonstrates again how significant a contribution research on women makes to our understanding of a particular time and place. It also delineates the challenges that women’s history poses and the benefits that flow from research that manages to meet them. Legendary as Taseko may have become, Walthall discovered early how incomplete the record of her life really was, and putting this book together meant going far beyond standard biographical approaches. Walthall reads against the grain of other interpretations but, most of all, fills in the contexts of Taseko’s life in a way that really does bring private and public together.

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VERA MACKIE. *Creating Socialist Women in Japan: Gender, Labour and Activism, 1900–1937*. New York: Cambridge University Press. 1997. Pp. ix, 252. \$54.95.

Vera Mackie has conceptualized her project around three processes: those of theorizing, organizing, and imagining. “The process of ‘creating socialist women’ . . . involved the theorization of the class position of women and political strategies for mobilizing women in the socialist and labour movements; the creation of organizational structures in order to mobilize women effectively in the labour and socialist movement; and the process of imagining women in new ways: as workers, comrades and activists rather than as wives, mothers and subjects of the emperor” (p. 155).

Mackie further explains that her narrative of the movement of socialist women from subjection to activism results from her desire to replace the imagery of feminine passivity with images of political transformation (pp. 169–70), and she has arranged her chapters along this trajectory. Thus, after an opening essay on “The Socialist Future,” she moves on to imperial subjects (gendered subjecthood), wives, mothers, workers, and, finally, activists—using well-chosen contemporary illustrations to begin each section.

Mackie explains that her definition of “socialist woman” is somewhat pragmatic. While she includes in her study the writings of women who identified themselves as socialists and who published in socialist journals, she limits her attention to women in the urban-based socialist organizations in the Tokyo region and concentrates on women in the so-called “legal” left. She begins her study with an analysis of the early socialist feminist Fukuda Hideko, drawing extensively on Fukuda’s autobiography (published in 1904). For Mackie, this work represents “an attempt to resolve the tension between becoming a woman and aspiring to a modern notion of selfhood which could include participation in public political activity” (p. 5). She also sees in Fukuda’s writings the search for a “speaking position” to articulate a feminist and socialist consciousness against the backdrop of nationalism, politics, and gender construction in Meiji Japan.

For this reader, the most poignant of the “speaking positions” appear in the “Workers” chapter. Here, Mackie uses representations of women in labor movement publications, representations that highlight the difficulties of women speaking as “workers.” She reproduces a song, for example, that appeared in a textile workers’ journal in 1926. Therein are images of caged birds, spring rain, fragile flowers, frail birds, and lonely stars, with this particularly emotional stanza: “I am a mill girl: a lonely star/Far away from my family/ Twinkling in the dark, night sky/A tiny star brimming with tears” (p. 119). Mackie accepts the meaning of oppression in such words, but she also poses the question: “Can these images of fragility and pathos . . . be used to imagine the possibility of shared political action, as the basis for a political movement?” (p. 119). This query complements her stand that rather than a “top-down” view of the state acting on women, one also can look at the state from the perspective of women and their attempts to influence state policies (p. 129).

The "Activists" chapter, too, is strong. Mackie's leitmotif of the tension between being a woman and simultaneously trying to be a political activist appears strikingly, for example, in the case of Sata Ineko, who used fiction to depict the relationships between men and women in the left-wing movement and the tensions in reconciling the positions of wife, mother, worker, and activist. Another dimension of this chapter is Mackie's use of historical figures to explain the Proletarian Party Movement and the Proletarian Women's Organizations. I commend her for the work that went into these as well as the narrative accompanying them. There is a wealth of information throughout the book, in her glossary, and in her bibliography. This will be the standard English-language work on this subject for some time.

Mackie concludes that the socialist groups' failure to present united opposition to Japanese actions in Manchuria in 1931 "was a sign of the gradual capitulation of the legal left" (p. 145). Moreover, calls for state assistance thereafter to support mothers escalated, owing to families suffering from the economic depression and because of the problems of surviving with male family members absent in the military. The Mother and Child Protection Act, which was promulgated on March 31, 1937, and became effective on January 1, 1938, reinforced again the unequal relationship between women and the state. With this act, Mackie sees women again "positioned as weak supplicants in need of 'protection', to be provided by a strong and benevolent State" (p. 147). The author has taken us full circle.

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PAUL H. KRATOSKA. *The Japanese Occupation of Malaya: A Social and Economic History*. Honolulu: University of Hawaii Press. 1997. Pp. viii, 404. \$35.00.

This history of wartime Malaya opens with intimations of a grand treatise that will overturn both scholarly and popular perceptions of a critical, controversial period in the region's history. Scholars have long treated the Japanese occupation "as a major watershed, an event that put an end to the old order and created a new." This view, Paul H. Kratoska argues, rests on assumptions unsupported by historical fact. "Popular understanding is . . . full of misconceptions," notably that the war ended British colonialism; the Japanese ruled by terror; the Chinese were hostile to Japan; the Malays collaborated; and the Indians were won over by the crusade for Indian independence (pp. 1–2).

By the end of these 400 pages, it is clear that Kratoska's aim is, in fact, more modest. "Stories of the occupation often highlight dramatic events," he tells us in his concluding pages, "but for many . . . it was a dreary time when they got along with their lives as best they could" (p. 347). In his careful documentation of this dreariness, the author eschews even mid-range

theory. Social historians such as Zainal Abidin b. Abd. Wahid and Cheah Boon Kheng have argued that the war gained a perverse significance as Japanese officials pitted Malay police against Chinese guerrillas, leaving a persistent legacy of communal division. Although Kratoska does not dispute their conclusions, even this modest attribution of significance seems to impose too much coherence on a period that was marked, in his view, by the social fragmentation, even confusion, of small lives divided in a mundane search for survival.

The sum of his succession of topics and anecdotes seems to say that a global war swept down this narrow peninsula, reducing all that might have been heroic, grand, or significant elsewhere to the inconsequential, even absurd, in this colonial backwater. The Japanese came to purge Asia of Western influence, instill discipline in local populations, and build a new economic order fully integrated with their imperium. But ignorant and overwhelmed by the peninsula's social complexity, the Japanese soon found themselves working feebly to facilitate mass survival in the economic ruins of what had been a prosperous, profitable colony. The Chinese, under the Malayan People's Anti-Japanese Army, tried to make the peninsula a frontline in the China War but—through betrayal, ineptness, and weakness—failed to accomplish anything of military significance. The Indian National Army, recruited and trained in Malaya under the charismatic Subhas Chandra Bose, marched off for glory on the Burma front and there suffered a humiliating, inconsequential defeat. Malay militants, led by Ibrahim Yaakob, had visions of mass mobilization and union with Indonesia that passed through brutal slaughters of Chinese farmers to political oblivion by war's end. Even Britain's triumphal return, planned with the precedent of the great Normandy and Leyte landings, started with trucks bogged in the sand and ended in a traffic jam. Within a decade, all these historical actors manqué had faded into inconsequence.

Instead of dramatic events, Kratoska focuses on the occupation's dreariness. Unable to absorb Malaya's major exports, rubber and tin, the Japanese tried to balance their administrative books with a mix of extortion, expropriation, currency inflation, and mass gambling. When the prewar economy collapsed, the Japanese military administration encouraged Malay small holders to cut down their rubber trees and pressed the Chinese to quit an untenable urban life for subsistence farming. Nothing really worked. As the author demonstrates with assiduous research and close analysis, Japanese corporations bungled, subsistence farming lagged, and cities became hollow shells. Everyone, save the Japanese and a few blackmarket dealers, went hungry. Average caloric intake dropped by half, and by war's end twenty-three percent of Singapore school children were "grossly undersized" (pp. 319–20).

Instead of glory or a grand political direction, the Japanese occupation left, for its survivors, trauma from the "everpresent threat of torture," an overarch-

ing "moral deterioration," and an "overriding selfishness." But with the passing of the wartime generation, even this mingled legacy is being lost. In recent histories and memoirs, "the singularity of the occupation has all but disappeared," and it has been reduced to "one episode among many in an eventful era" (pp. 346–61). Kratoska ends his preface with the hope that his son "will never have to live through anything comparable to the events described in this book" (p. vii). It is a curious yet creative history that is written not to recall or redefine but instead to consign to oblivion. The Japanese occupation of Malaya? Forget it, this author seems to say.

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SANDRA C. TAYLOR. *Vietnamese Women at War: Fighting for Ho Chi Minh and the Revolution*. (Modern War Studies.) Lawrence: University Press of Kansas. 1999. Pp. x, 170. \$29.95.

Considering that at least ninety percent of all studies of the Vietnam War in English concentrate on the Americans, one should never ignore a book on the Vietnamese experience. Also, among the publications about Vietnamese protagonists that do exist, female combatants have received scant attention indeed. Sandra C. Taylor's book aims to address this lacuna.

Over a number of visits to Vietnam, Taylor interviewed scores of female veterans of the twenty-year-long struggle against forces of the Republic of Vietnam and the United States. These encounters clearly moved the author deeply and provide the main justification for the book being published at all. Of most value are her interviews with southern rural women, who faced incredibly diverse challenges month after month and often had to devise their own solutions with no help from higher echelons. These quiet achievers at the village level also tended to be ignored by communist agitprop cadres (mostly male), who preferred to portray women as either martyrs to enemy violence or lofty national heroes.

The author's interviews were arranged by the official Vietnam Women's Union, she had to rely entirely on interpreters, some of the veterans had polished their stories with multiple tellings, and no one strayed from the party line. Taylor notes all this in passing, yet she still takes far too much of the interview evidence at face value, without trying to corroborate it elsewhere. In many cases, she seems to have become part of a local ritual performance, with her role preventing the asking of critical questions that might embarrass her hosts. Probably only a trained anthropologist, fluent in Vietnamese and willing to spend weeks with individual veterans and their families, could get beyond all the politically correct generalizations to the complex, idiosyncratic accounts lying beneath.

Mindful that writing a history requires more than interview transcripts, Taylor did visit a number of

museums in Vietnam, perused Hanoi's English-language newspapers of the period, and canvassed some Rand Corporation prisoner interrogation reports and captured document translations now housed at Texas Tech University. Unfortunately, she almost never uses these alternative sources to cross-check evidence obtained in her interviews. For example, contrary to what she was told, the Rand interrogations demonstrate that some captured women did betray the whereabouts of their comrades and reveal other secrets to the enemy. Battle victory claims by interviewees could have been compared with accounts in U.S. unit histories or daily casualty figures: for example Pham Thi Tao's assertion that her eleven-girl unit killed 120 GIs in one engagement (p. 74).

Ultimately, this is more a book about memory than history. The author communicates to us the inner strength, resiliency, ideological justifications, and selective recollections of the elderly women she encountered. We also learn something about the conflicting roles facing Vietnamese women since the end of the war. In most cases, patriarchy and a culture of female self-denial have prevented women veterans from capitalizing on their wartime achievements. As for history, Taylor admits at one point that "The stories might not be literally true, but they represent what people want to remember and what serves their present needs" (p. 18). From such a statement it is only a short step to abandoning the historian's craft.

During the past twenty-five years, hundreds of Vietnamese-language memoirs, unit histories, documentary collections, and works of fiction about the war have been published. Archives in Hanoi and Ho Chi Minh City are now accessible to foreign and Vietnamese researchers alike. Armed with data from these print sources, serious scholars will be able to ask more precise, probing questions of surviving veterans. Hopefully at least one of those scholars will take up the topic of women combatants and political activists. In the meantime, one can only agree with the last sentence in Taylor's book: "Clearly, my work is but the tip of an iceberg" (p. 165).

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ANTONY COPLEY. *Religions in Conflict: Ideology, Cultural Contact and Conversion in Late-Colonial India*. New York: Oxford University Press. 1997. Pp. xvii, 279. \$32.00.

The work of missionaries is conventionally perceived as that of public preaching. From pulpit sermons to televangelism, the missionary is historically a presence in public places, spreading the word of God by oratory and audience capture in places as diverse as bazaars, parks, and street corners. Itinerating, or the movement from place to place, is regarded as intrinsic to the missionary's vocation, so much so that the missionary is seen as the prototypical explorer, prefiguring the colonialist adventurer. If, in time, the work of Chris-

tianization dovetails with the work of empire, the missionary's role must be seen as key in charting the path to global domination.

Why, then, when empire was so successful in consolidating the hold of Christian countries over non-Christian ones, did the missionary enterprise of preaching by itinerating constantly struggle to sustain itself, often diverting its resources to other activities such as social reform and education? This was particularly true in colonial India, where Christianity had to take on religions so deeply entrenched in the social system that missionaries found the only way they could communicate Christian teachings was, paradoxically, by submitting themselves to a strenuous course of studies in Indian religions. Driven by a spirit of Christian apologetics, missionaries proved to be not only explorers but also proto-Orientalists, turning the study of indigeneous traditions into a demonstration of Christianity's superior doctrinal merits. The fact that the work of missionaries was engaged by a range of activities taking them away from a straightforward dissemination of Christian truths—handing out Bibles at street corners being its most uncomplicated expression—suggests how stressful was the process of missionization. The success rate in gaining converts was never quite proportionate to the efforts expended.

Antony Copley's book surveys the shifts in missionary purposes in nineteenth-century India, concluding that the latter part of the century marked a departure from itinerating to education. Such a shift, notes Copley, was a concession to the diminished returns on door-to-door preaching and the fact that education offered strategic access both to the lower castes seeking escape from the debilitating hold of Hinduism and the upper castes desiring the accoutrements of Western culture. If missionaries were drawn into the work of education rather than itinerating, it is a measure of the greater social reach offered by educating young Indians, who looked to selfless missionaries as alternative father figures. Although homoeroticism is a muted theme in Copley's book, there are strong suggestions that the missionaries presented a model of gentleness and effeminate caring that attracted young males to them, especially those burdened by the strictures of Hindu patriarchy. Indeed, intense personal relations between missionary and pupil accounted for far greater success in gaining converts than the most ardent preachings in bazaars and other public places.

To illuminate these connections and shifts, Copley divides his book into three sections: first, a critical overview of the main issues in Indian missiology; second, a regional examination of English missionaries working in North, East, and South India; and third, a study of Indian converts in the same three regions of India. The book's chief contribution lies in its account of regional differences in the growth of missions and Christian conversions. At times, however, the short sketches of missionaries and converts have too much of a profile-oriented approach that obscures the enun-

ciation of broader themes and developments, particularly those related to colonial policy and new alignments of metropole and colony. Nonetheless, Copley's reading of the missionary archives yields fresh documentation of the ongoing tension between itinerating and education. Even though education may have superseded itinerating in the 1880s, this tension was never effectively resolved. Following the disastrous Mutiny of 1857, itinerating allowed missionaries to project their own narrative of events, contesting the official view that their zeal provoked the rebellion. One might add that even the intense missionary desire to ghost-author converts' life stories must be related to the same urge to control the narrative point of view of mission.

Refusing to glamorize the missionary's role, Copley points out that shifts in missionary purposes were often in response to their potential constituency's interests, which frequently revolved around property and land claims. By drawing attention to the tribal population's shrewd enlisting of missionaries as allies in the defense of their tribal lands, the author provides strong evidence to suggest that Indians used missionaries as much for their own material purposes as missionaries did Indians to achieve their theological goals. The case can be made even more strongly than Copley does that not only did missionaries move from itinerating to education, but there was a further shift to law, which shows how readily missionaries adapted their original objectives to arenas where they could expect more success. Indeed, by the time the Caste Disabilities Removal Act (which protected converts against loss of rights to property on conversion) was passed in 1850, missionaries had shifted the tenor of their proselytizing rhetoric to become advocates of converts' rights. Copley interprets the passing of new laws protecting converts as the colonial state's capitulation to missionary aims. This view does not adequately consider that the colonial state had its own objectives, which were not necessarily identical with those of the missionaries. Disabilities legislation, while seemingly favorable to the work of mission, asserted one of the central premises of English liberalism—inalienable rights of property—and therefore emerged from the tenets of English political doctrine instead of merely being a response to missionary pressures.

Of central importance is the discontinuity between the goals of the colonial state and missionary institutions. So sharp were their differences that missionaries saw that they had to engage a two-pronged attack against Hinduism, on the one hand, and the utilitarianism of the colonial state, on the other. In most instances, Copley is sensitive to the tense relationship between administrators and missionaries, and he points out an important fact often ignored or misunderstood: the colonial state did not see its fundamental goal as one of Christianizing the colonized but rather favored policies of anglicizing their subjects; in other words, making them good English men and women rather than good Christians. One of the ironies of the

missionaries' shift toward education is that it forced them to adopt anglicization as a tandem activity of Christianization, even though theoretically they favored the study of vernacular languages and literatures.

Although Copley attempts to give a sense of converts' private compulsions, he is more successful in locating Christian converts within the reform movements of nineteenth-century India. In the context of a revivalist logic that claims Hindu society can be open to Western science without sacrificing its own traditional culture, converts were an anomaly. By converting, they asserted the possibilities of another narrative of social change that was at variance with the selective synthesis of East and West attempted by Hindu reform movements. However, Copley's assertion that converts were confused individuals because they betrayed their own cultures reflects the perception of Hindus rather than converts themselves. There is surely a danger in reading converts as if they are interchangeable with the alienated products of English education, for although it is true they were often caught between competing loyalties, converts experienced crises of identity arising as much from a recognition of the gap between Christian belief and the "Christian" empire as of that between Hinduism and Christianity. In the case of a convert like Pandita Ramabai, this recognition allowed her to align herself with English dissenters opposing the English state, which translated for her into an anticolonial stance, even as she detached herself from Hinduism. Such a double focus allowed converts to critique British colonialism more presciently than the English-educated elite were able to, a view with which Copley himself might agree when he suggests that converts were in the vanguard of resolving a cultural crisis induced by anglicization.

The author's overt focus on elite classes self-consciously avoids consideration of the mass conversions that involved lowcaste groups. Yet it is curious that, despite the strategic importance of the uppercastes for mission work, Indian Christians saw their main allies as outcastes and lower castes rather than Brahmin intellectuals. This is a contradiction in the development of Indian Christianity that is well worth exploring further. Given Copley's focus on the shift from itinerating to education, it would have been helpful to learn more about the interaction between the mission schools and the government institutions, like the famed Hindu College in Calcutta that produced Bengali rebel-intellectuals. These two types of colonial institutions replicated the divide between religion and secularism and became the ground for defining the terms of Indian modernity. Copley implies that moving away from itinerating made missionaries more open to Indian culture and religion, but there is little evidence to suggest this was indeed the case. As the author himself ruefully observes, the great Scottish missionary and educator Alexander Duff gives the lie to that theory, for Duff's educational mission hardened the

lines of Anglicism and engendered exclusivist attitudes toward Indian culture.

Copley's work adds much needed regional detail on the development of Protestant missions in late colonial India. Yet its conclusion that missionary failures attest to the resilience of traditional institutions minimizes the role of reform movements far more than the evidence suggests, and it leaves Hinduism fairly untouched by missionary incursions into its practice. Poor editing mars an otherwise scrupulously researched book: appalling mistakes abound, such as referring to Wilhelm Halbfass as Halbermas (p. 254).

GAURI VISWANATHAN
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ANAND A. YANG. *Bazaar India: Markets, Society, and the Colonial State in Gangetic Bihar*. Berkeley and Los Angeles: University of California Press. 1998. Pp. xi, 305. Cloth \$55.00, paper \$25.00.

Space and spaces, place and places, and the interconnections therein and between are often forgotten dimensions in historical writing. Some recent writing, however, has taken an interesting spatial turn—or return since it is a revival of what some previous historians knew well and expressed effectively, namely that time(s) and space(s) need to be explored together. With its emphasis on markets and their sociocultural and economic interconnections, Anand A. Yang's book has a decidedly spatial orientation.

This is a fine, innovative book. Clearly written and well researched in an impressive body of indigenous and colonial sources, it much improves our understanding of the changing horizontal and vertical linkages of South Asian societies under colonialism. Many markets of various sizes and functions provided, in Yang's analysis, the crucial synapses where economic, cultural, and political interests (subordinate and superordinate, "British" and "Indian," and varieties of both) meshed and contested one another. The geographical and temporal context of the book is a 24,000-square-mile area north and south of the city of Patna in the middle Gangetic valley from the later eighteenth century to the 1940s. The specific context notwithstanding, however, the book deserves to be read widely. One does not need to specialize in colonial India in order to follow Yang's arguments and to benefit from his book's content.

The substantive heart of the book is contained in chapters two through four, where Yang examines the workings of three varieties of sites at which exchange took place. Each category of site, moreover, is examined within a particular time frame, which overlaps but is not synchronous with other chapters. Time, in this book, has an imbricated rather than a sequential presence. Spatially, Yang uses central place theory in a fluid, nondeterministic way to help organize parts of his presentation: his sites do fall into a hierarchy, but they also overlap. The fifth chapter discusses some of the social implications of the changes in bazaar (mar-

ket) India: for example, the success of some trading and merchant groups and the failures of others.

The first site, Patna, is the central place of the region. As Yang describes the evolution of Patna from the later eighteenth century, a complicated story emerges of declining fortunes and eroding economic centrality—in part because of British-sponsored developments in the realm of transportation—counterbalanced by the continued political and administrative importance of the site. The physical development of Patna reflected these changes, as did the fortunes of old and new elites within the city.

Chapter three examines religious places of exchange, especially *melas* (religious fairs sometimes held at centers of pilgrimage), in order to situate them within the marketing system. Yang explores the various ways in which *melas*, periodic though they were, provided an established occasion for economic exchange—large-scale economic exchange with translocal links in the case of great *melas*—and an opportunity for religious celebration and other enjoyments. Livestock markets, for example, often were held at *melas*. Religious fairs, of course, had sociopolitical qualities as well: important figures in the countryside patronized fairs to enhance their claims to high status and to benefit financially, and the British worried about the subversive potential of such gatherings.

Chapter four looks at rural places of exchange, with a particular eye on the configuration of sites that had emerged by the 1920s. Yang discusses periodic markets (*haats*), standard markets, intermediate markets, and central markets, and notes the increased symmetry of the system. But he is most interested in the bazaars as the loci of much nationalist political activity in the era of the Gandhian Non-Cooperation Movement to whose peripheral locations, Yang argues, “historical initiative” (p. 160) had passed.

In sum, this an excellent book. Some readers might like Yang to engage explicitly the theorizing about space provided by Henri Lefebvre, David Harvey, Edward Soja, and others. Space(s) and place(s) are at the heart of the book. In addition, some of the postcolonial musing in the introduction—muted in the subsequent chapters—could have been left out. Nonetheless, there is little in this book that I do not recommend. A detailed review could take issue with particular aspects, but they would not detract, as the stockbrokers say, from a strong buy recommendation.

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CANADA AND THE UNITED STATES

MARC EGNAL, *New World Economies: The Growth of the Thirteen Colonies and Early Canada*. New York: Oxford University Press. 1998. Pp. xix, 236. \$49.95.

Marc Egnal compares economic development in the Thirteen Colonies and Canada from 1665 to 1783. Using a modified staple approach, Egnal suggests that

changes in Britain and France influenced long swings in colonial export sectors in terms of trade and capital flows. Metropolitan cycles further affected “additions to the capital stock, gains in productivity, and patterns of culture” (p. 7).

Colonial economic growth varied regionally. Gradual economic expansion in Britain (1713–1745) and rapid expansion (1745–1783) provided the consumer demand and credit that allowed the Thirteen Colonies to grow. Between 1713 and 1730, northern agriculture suffered due to poor West Indies markets. Farmers did not invest in productivity, and colonial shipping slumped. Between 1745 and 1760, rising prices for West Indies sugar allowed planters to demand more northern foodstuffs. Southern European and continuing British demand for foodstuffs and other semi-processed industrial inputs ensured slower growth after 1760. Agricultural productivity and colonial manufacturing increased, although credit contracted and shipping suffered as the West Indies trade declined after 1760.

The Upper South’s economy grew more slowly than the North’s from 1732 to 1775. From 1713 to 1731, wars and speculation crises weakened European demand for colonial tobacco, which producers shipped through British ports. The recovery of European markets after 1730 partially revived the tobacco economy, although disruptions due to wars continued. Three factors, unrelated to demand, compensated for the problems of war: higher quality produced by colonial inspection laws, Scottish merchants’ efficient reorganization of the tobacco trade, and greater planter investment in slave labor. Landowners used grain production on exhausted soils to compensate partially for downturns in prices for tobacco. Grain sold mostly in colonial markets, strengthening local craft production.

Subregional differences marked the Lower South’s production of rice, indigo, and, to a lesser extent, naval stores and deerskins. Continental European demand for rice led to robust economic development in South Carolina and Georgia from 1713 to the early 1740s but withered during the War of the Austrian Succession. Demand for rice revived in the late 1740s and took off after 1765 due to increasing demand in Dutch markets. Planters improved productivity by shifting rice cultivation to wetlands. Diversification by indigo production and the increasing availability of British investment funds helped planters counteract periodic downturns in longer periods of growth. North Carolina’s tobacco, naval stores, and lumber trades, hampered by poor harbors, followed similar but weaker growth patterns.

French state policies more than market factors affected Canadian economic development. Crown regulation buoyed the fur trade from 1665–1672. State support for the military and religious establishment encouraged urban development and provided additional markets for colonial farmers and artisans. Agriculture benefited from state-supported immigration. In the early 1670s, reductions in French expenditures slowed development, but the economy rebounded as

the fur trade and agriculture grew. Canada began to supply some of the French West Indies trade. From 1689 to 1713, reduced government spending due to war, overproduction of furs, weakened demand for furs, and growing colonial conflict with the First Nations led to economic depression. From 1713 to the early 1740s, the fur trade and farming grew steadily as the crown spent more on imperial trade. A greater military establishment provided markets and protection for colonial farmers and fostered Canadian trade with the West Indies. Commercial activity in the West Indies and fur trades lagged behind government spending in Canadian economic growth. Military spending reflected the growing conflicts with the British which, by the 1750s, began to disrupt "the long swing of Canadian expansion" (p. 165).

Egnal's presentation of evidence is exemplary. His tables, graphs, and other figures elegantly simplify much quantitative evidence from a variety of secondary and primary sources. The correlations between European markets and policies and colonial industries are clear. Less clear are the causative implications of these correlations. For example, Egnal links growth in commercial and artisanal activity in South Carolina and Georgia to general growth in staple trades, but without much explanation. Similarly, there is little explanation for rising productivity in northern farming after 1745 (p. 65). Egnal's work seems much like previous staple theories: good markets for staples produced a lot of other indicators of growth in the colonial economy. Even Egnal's suggestion of the varying importance of commercial and state factors in the Thirteen Colonies and Canada echoes Harold A. Innis's classic, *The Fur Trade in Canada* (1930). As Egnal suggests in his preface, readers who desire more analysis should consult his earlier work, *Divergent Paths: How Culture and Institutions have Shaped North American Growth* (1996).

SEAN T. CADIGAN
Dalhousie University

JULIAN GWYN. *Excessive Expectations: Maritime Commerce and the Economic Development of Nova Scotia, 1740–1870*. Montreal: McGill-Queen's University Press, 1998. Pp. xvi, 291. \$55.00.

When an indefatigable historian puts in years of work in the archives and writes up the findings in a straightforward manner, the results are useful for the community of scholars in that field—no matter what the current fashions in the discipline of history may be. That is the case with Julian Gwyn's book on the economic history of Nova Scotia. The tables alone are testaments to his digging ability. There are seventy-two of them, ranging from "Commodity prices, unweighted index, 1773–1815" to "Balance of Payments: capital account, annual averages, 1850s and 1860s." It must have been an immense labor to identify, collect, and organize the material that makes up these numerate snapshots of the Nova Scotian economy over more

than a century. As Gwyn would be the first to acknowledge, the basis for these tables is often shaky, since neither the local colonial authorities nor the imperial government kept figures in convenient categories that would give a complete picture of all the local, regional, and coastal economic and trading patterns that together constituted the Nova Scotian economy. Simply putting this big picture together is an achievement in itself, despite lingering questions about the reliability of some of the figures. For purposes of analysis, Gwyn divides his task into two parts. In the first, he surveys three distinctive periods (1740–1815, 1815–1853, and 1853–1870), emphasizing the extent to which Nova Scotia's fleeting periods of prosperity depended on what was happening elsewhere in the British Empire and the Atlantic economy. In the second part, he looks at local economic regions, the relationship between imports and the standard of living, the impact of reciprocity, and the balance of payments as devices for evaluating economic development—or non-development. Even though some specialists might question aspects of his analysis, this is the most comprehensive and thorough account of the Nova Scotian economy for this period that we have.

The main conclusions are not radical, but they are bold enough to challenge the conventional wisdom in some key areas. Gwyn argues that Nova Scotia's marginal geographical position and limited natural resources precluded anything other than an economy that was derivative and poor. He is at his boldest when he punctures the myth of a golden age in the 1840s, when Nova Scotia's wooden sailing vessels carried a not negligible portion of the world's trade. Even this blip of prosperity, claims the glum Gwyn, was a mere accident. Local shipbuilders could not sell their ships to Britain during a troubled economic era and were forced to retain possession and take up freighting. No sooner had this role as an international carrier gathered some momentum than it began to be undermined by the introduction of steel and steam ships. And even as shipping took an upward turn, the agricultural sector was experiencing depression. There was, in sum, no pre-Confederation era of plenty in Nova Scotia. Nova Scotia's perennial economic frailty cannot therefore be put at the door of post-1867 Canadian exploiters but reflects the geographical and historical circumstances that have condemned the colony and province to marginality.

It is a vigorously argued case but tightly focused on economic lines of investigation. This is odd in view of the fine series of contemporary sketches, paintings, and lithographs reproduced (pp. 125–129). These portray general views of substantial towns, bustling harbor scenes, idyllic agricultural landscapes, and busy market places. Such positive images are at odds with the gloomy analysis in the text. This disjunction between text and illustrations raises the question of how the past is represented to us. This question is a hoary one in the discipline of history and has been reinvented in fundamentally new ways by postmodern scholarship. It

does not figure at all in this book, which is a pity, for in the end only a multiperspective approach that includes religion, politics, and culture can tell us how Nova Scotia's history was experienced. Gwyn's lens is too narrowly focused to provide satisfying answers to the issues he raises about the relationship between expectations and identity. Anyone who attempts to present that multifaceted view of Nova Scotia's past, however, will not be able to ignore Gwyn's contribution.

GORDON T. STEWART
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E. A. HEAMAN, *The Inglorious Arts of Peace: Exhibitions in Canadian Society during the Nineteenth Century*. Buffalo: University of Toronto Press. 1999. Pp. viii, 412. \$50.00.

Attempts to improve agricultural methods and products in central Canada date almost from the start of the invasion of English-speaking settlers into the region. Beginning with agricultural societies, these efforts soon adopted exhibitions as a supposedly effective tool. Designed to teach better agricultural methods through competition and display, exhibitions quickly grew in popularity and eventually mushroomed into large international showcases. In her closely argued, exhaustively researched work, E. A. Heaman claims that exhibitions reflected the nineteenth century's obsession with economic progress and the belief that material and cultural advancement could be gained through competition and education. Citing John Stuart Mill, Heaman posits that Victorians believed advances in knowledge preceded all significant physical improvements. Furthermore, organizers designed their exhibitions on the assumption that visual displays were better than words. By arranging artifacts in order, they thought, learning would happen.

The altruistic objectives of exhibitions—encouraging rational and critical activity, facilitating improved methods and products through competition—were marred, Heaman contends, by a naïve belief in the ultimate good of capitalism, advertising, and the profit motive. Not only did exhibitions disseminate upper-class values, but they appealed to covetousness, jealousy, and materialism. The fact that exhibitions were also about money, power, knowledge, and influence meant that they became very different institutions than their founders intended. By appealing to greater public participation, by advertising and providing popular entertainment, the exhibitions became increasingly frivolous and debased.

Heaman organizes her book into two large sections. The first deals with the history and culture of exhibitions in central Canada. It notes the differences in development between the French and English-speaking regions and traces how exhibitions reflected and exacerbated the tensions between these cultures and between political ideologies within each province. Instead of uniting classes and cultures, exhi-

bitions tended to accentuate differences and encouraged political dissent. Intended to trickle knowledge down from the top to the bottom, exhibitions in fact encouraged public participation, educated people, and permitted them to undermine this principle. Heaman convincingly demonstrates that exhibitions, as they strove to appeal to popular culture, gradually became more and more show-like or carnivalesque.

The second part of the book deals with the large international exhibitions. Canadian participation in these was controlled fully by the state and had as its purpose self-advertising and profit-seeking. The intent was to attract immigrants to the country, open markets for farmers and industrialists, and increase the international stature of the fledgling nation. In the pre-Confederation era, the author asserts, Canadian exhibits at foreign capitals helped shape British North America's consciousness as a geographical entity and contributed to the Confederation process. In the post-Confederation period, Canadian participation in international expositions helped shape its identity as a community.

Almost as an appendix, Heaman adds a third section to her book. Consisting of two chapters, one dealing with women and the other with Native peoples, this part expressly reinforces postmodernist thinking that issues of gender and race must be discussed outside mainstream history because they erode traditional concepts and categories. Although this may be true, treating the increasingly important role that women and aboriginal citizens played in the culture of white male-dominated exhibitions in the first two sections would have created a more holistic, integrated story.

The strength of this book lies in its firm grounding in European and North American intellectual history and its artful integration of exhibitions into their contemporary political contexts. Heaman's argument about how exhibitions arose out of nineteenth-century political, economic, and philosophical thought is thoroughly convincing. Undoubtedly, the exhibitions were a reflection of contemporary culture. Less persuasive is Heaman's suggestion that exhibitions had a significant impact on larger political developments like the rebellions of 1837–1838, Confederation, and national consolidation. The notion that exhibitions—along with other intangible factors, such as free trade, more efficient means of transportation, and advances in science—contributed to the centralization and unification of Canada is plausible, but the extent of their contribution remains conjectural. Perhaps the mid-nineteenth-century nationalist movements were, like the expositions, products of an expansionist ideology.

That quibble aside, this book is a very important addition to the growing body of literature on nineteenth-century Canadian society and culture.

A. A. DEN OTTER
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NINETTE KELLEY and MICHAEL TREBILCOCK, *The Making of the Mosaic: A History of Canadian Immigration*

Policy. Buffalo: University of Toronto Press. 1998. Pp. viii, 621. Cloth \$75.00, paper \$29.95.

Ninette Kelley, a member of the Canadian Immigration and Refugee Board, and Michael Trebilcock, a professor of law and economics, tell us that much current public policy analysis is concerned with whether ideas, interests, or institutions are the more influential determinants of policy outcomes (p. 4). They adopt this approach of public policy analysis in their examination of the history of Canadian immigration policy from European arrival in the territory of present-day Canada to the 1990s. "Interests" refer to material self-interests such as employers' desire for cheap immigrant labor and organized labor's fear of competition from immigrant workers. "Ideas," seen as interchangeable with values, refer to the non-economic determinants of immigration policy such as racism and political ideology, and "institutions" are the elected and administrative arms of the state and the law.

The book's analysis of immigration policy begins with a brief survey of developments prior to Confederation, followed by eight chapters examining immigration policy since Confederation in greater detail. The authors believe that between 1867 and the 1940s, racist and anti-radical ideas gradually modified a policy dominated by business interests. The use of racist criteria in the selection of immigrants came under attack in the wake of World War II, but new, universalistic criteria were not adopted until the 1960s. In more recent years, Canada's refugee policies have been chiefly responsible for rendering the question of immigration contentious once more. Because they believe that a conflict between ideas of liberty (of the individual prospective or actual immigrant) and conceptions of community within the host society is at the heart of the debate over immigration policy, Kelley and Trebilcock extend their analysis to the admission of immigrants and the members of ethnic minorities into the Canadian community through naturalization, or their exclusion from that community through discrimination and deportation.

The book's description of the main historical developments in Canadian immigration policy resembles two other recent overviews of that policy: Valerie Knowles's *Strangers at Our Gates: Canadian Immigration and Immigration Policy, 1540-1990* (1992), and Donald Avery's *Reluctant Host: Canada's Response to Immigrant Workers, 1896-1994* (1995). Indeed, despite the difference in terminology, Kelley and Trebilcock's analysis of the forces shaping immigration policy is not unlike Avery's explanation of the influence of pressure groups such as business, labor, and bureaucrats on immigration policy. What distinguishes this book from the two earlier studies is its authors' perspective as policy analysts. They explore the significance of executive and administrative action in new, interesting, and more detailed ways. We learn how the Department of Citizenship and Immigration restricted debate over

immigration questions. Until the 1950s, for example, it produced annual immigration figures and estimates on the last day of a parliamentary session. The book also offers the first full-scale examination of the role of the courts in shaping immigration policy. Using court cases, many of which have not previously been analyzed by students of immigration history, the authors show that until the mid-1980s, Canadian courts did not generally challenge executive and administrative decision makers, even in cases where immigrants were denied due process.

Historians will undoubtedly benefit from the book's clear and well-documented exploration of the role of institutions in shaping immigration policy, but they may find its concern with models employed by policy analysts baffling at times. While the conceptualization of "ideas and values" and "interests" as independent variables in explaining policy development may be a widely accepted practice of policy analysts, for example, historians may find the use of such classification somewhat arbitrary. One illustration offered here of the "substantial independent explanatory power" of ideas and values with respect to features of the Canadian immigration policy is the influence of "ideological hostility to collectivism in the organization of the economy" in the expulsion of political radicals from Canada in the 1920s and 1930s (pp. 448-49). But can this ideological hostility really be divorced from the economic interests of Canadian employers of immigrant workers? How much independent explanatory power should one accord to the triumph of "liberal values," as opposed to Canada's changing immigration needs and the reduced availability of immigrants from traditional source countries, for eliminating explicit racial and ethnic criteria in the selection of immigrants since the 1960s? Because historians do not generally employ the same criteria as public policy analysts, they may find that Kelley and Trebilcock's conclusion that the making of Canadian immigration policy is "too complex to be accounted for by a single theory of determinants of public policy outcomes—ideas and values, interests and institutions all shape these outcomes in complex and interactive ways" (p. 450) does not do justice to the richness and complexity of their study of the development of that policy.

CARMELA PATRIAS
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MARY-ELLEN KELM. *Colonizing Bodies: Aboriginal Health and Healing in British Columbia, 1900-50*. Vancouver: University of British Columbia Press. 1998. Pp. xxiii, 248. \$75.00.

Although Mary-Ellen Kelm begins this book by promising to explain how bodies are fashioned by colonial forces, what she delivers is an informative, if inconsistent, survey of documents dealing with disease and health care in what is now British Columbia, Canada's westernmost province. Kelm is to be praised for interpreting Canada as a minor imperial power undertaking

a colonizing mission within its own borders. Her book, however, is an example rather than a critique of colonial discourse.

Part one, "Health," reviews the impact of colonization on the health of the First Nations of British Columbia. In chapter one, Kelm tries but fails to free herself from a colonial discourse that types the aboriginal people of North America as a vanishing race. The chapter is a ghoulish survey of causes of death, population growth and decline, mortality rates, and hospital admissions. Colonization not only made the First Nations into specters of death but also stripped them of control over resources, and in chapter two Kelm describes how, for example, the imposition of the reserve system and the industrialization of fishing affected diet and nutrition in aboriginal communities. Chapter three traces the relation between health and space. Missionary "influence" encouraged aboriginal people to build cramped and poorly ventilated houses that facilitated the transmission of infectious disease—as did the living conditions at the canneries and hopfields where many aboriginal people worked as wage laborers. Reserve communities had to fight to hook up their houses to water lines and sewer systems. Chapter four recounts how the residential school system in British Columbia "transformed" the bodies of aboriginal children. Judging from Kelm's careful accumulation of texts, the preferred technique for transforming scholars' bodies was simply to kill them. School buildings built with the humanitarian intention of "improving" aboriginal people were scenes of violence and avenues for the transmission for bronchopneumonia and tubercular meningitis. As Kelm observes, each year "scores" of tortured and undernourished children were discharged from school because they were not expected to live.

Part two, "Healing," discusses the impact of Western allopathic bio-medicine on the healing practices of First Nations. In chapter five, using European concepts to describe non-European practices, Kelm says the work of healing mediates between a "human" and an "inhuman" world. She also comments on "witchcraft," a concept she declares "European" but uses anyway. Chapter six reviews the half-hearted efforts of the Department of Indian Affairs (DIA) to deliver health care to aboriginal communities. Hospitals were racially segregated, and settlers worked with the DIA's Indian Health Services Branch to attract doctors to remote communities to treat white patients. Kelm documents the practice of "medical pluralism" in chapter seven. She observes that aboriginal people have long demanded equal access to bio-medical treatment while continuing to practice their own modes of healing. As late as the 1940s, however, women in labor were admitted to a racially segregated sanatorium built to treat people with tuberculosis.

Since the British Columbia First Nations are among the most talked-about people on earth, Kelm makes a point of citing texts by aboriginal people. She often cites her own interviews with elders. Yet, twenty years

after the publication of Edward Said's *Orientalism* (1978), Kelm fails to adopt a critical stance toward the colonial archive that supplies the bulk of her "data." While she concedes that government documents and nineteenth-century ethnographies offer "filtered descriptions" of aboriginal societies, Kelm cites those documents as if they gave unfiltered access to "aboriginal bodies." Although she cites Michel Foucault, she ignores his claim that the body is an artifact of discourse. She consistently elides the distinction between *what was* and *what was said*.

Kelm's book illustrates the danger of failing to read colonial discourse as discourse. Kelm notes in her introduction that colonial archives have long spoken of aboriginal people as if they were, "by nature, sick" (p. xvi)—a superb insight that could have led to a ground-breaking study. Unfortunately, the book that Kelm has written falls back into the very discourse that she begins by critiquing. Although she makes many texts available for study, and despite her obvious good intentions, this book shows that what white audiences continue to want to know about First Nations in "Canada" is how they fall ill and how they die.

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MARILYN C. BASELER. *Asylum for Mankind: America, 1607–1800*. Ithaca: Cornell University Press. 1998. Pp. xi, 353. \$42.50.

MARIANNE S. WOKECK. *Trade in Strangers: The Beginnings of Mass Migration to North America*. University Park: Pennsylvania State University Press. 1999. Pp. xxx, 319. Cloth \$60.00, paper \$21.50.

American revolutionaries did not so much invent the idea that the new nation would serve as an asylum of liberty for oppressed peoples of the world as ratify a notion that was as old as the colonies themselves. These two studies, using different approaches, establish the primacy of the colonial period in laying the foundation for America's subsequent experience with immigration. At the same time, they demonstrate that America's ambivalence about its role as refuge has an equally long history.

Marilyn C. Baseler makes this ambivalence the central theme of her book. In dense and often fascinating detail, she locates the origin of the idea of America as asylum in seventeenth-century England. The island nation had gained a reputation as a refuge for persecuted continental Protestants, but worries over an influx of indigent immigrants encouraged officials to shift the responsibility to the colonies. Mercantilist thinkers strongly advocated such a policy, arguing that England only weakened itself when it allowed its own subjects to move to America. By redirecting the flow of continental Protestants to the colonies, England peopled its possessions while maintaining both its domestic population and its reputation as an asylum.

Over the next century and a half, the colonists—all of whom, of course, were immigrants or the descendants of immigrants—debated the role of their land as an asylum in terms that would become all too familiar to later generations of Americans. Free white Protestant Europeans (especially English ones) were more than welcome to enjoy the blessings of economic opportunity and mild government, while others—the poor, the Catholics—need not apply. Transported British convicts presented a dilemma: colonists could use the labor, but they resented England's identification of America as a convenient dumping ground for its own undesirables. Involuntary African immigrants sparked even greater controversy. Although their race defined Africans as "undesirable," only a handful of colonists could imagine dispensing with slavery. The irony, Baseler notes, is that fears of a rising slave population led to subsidies for white immigrants to maintain racial balance; thus the chances for advancement of the latter group depended on the subjugation of the former.

Immigration all but ceased during the Revolutionary War, but the debate resumed in the postwar period as a crucial part of the process of national self-definition. When British commentators denigrated the whole idea of an American refuge for the oppressed by citing the treatment of Loyalists, the former colonists protested that voluntary allegiance to republican ideals stood at the heart of American citizenship (which necessarily excluded Loyalists) and pointed to the renewed flow of immigrants—even from Britain—as evidence that America's reputation as an asylum remained intact. Yet such defenses of America failed to represent the full spectrum of its citizens' opinions. During the 1780s, Americans argued not only about the proper treatment of Loyalists but also about which immigrants were desirable and which were not. If citizenship was, in the end, easier to obtain in America than in any Western European country, unnaturalized immigrants initially confronted a welter of requirements and restrictions depending on where they settled.

It was in the 1790s that immigration policy first became a political football at the national level. At the start of the decade, Federalists defended immigration on mercantilist terms, as a way to strengthen the nation; Republicans, although they defended the idea of an American asylum in principle, feared that a too-rapid maturation of American society would lead to corruption. By the end of the decade, party positions had reversed. The Federalists' anxieties about an influx of French Jacobins quelled their enthusiasm for immigration and led to passage of the Alien Acts. The Republican outcry against such measures incorporated a vigorous defense of America's duty to shelter the oppressed, even as it reflected an equally powerful concern about the inappropriate use of federal power. Here Baseler offers an important corrective to the usual depiction of Republicans as the champions of the immigrant. While it is true that, after 1800, the Republicans eased some of the harsher Federalist re-

quirements for naturalization, Baseler reminds us that they preserved such convenient instruments as the Alien Enemies Act, which they used to discriminate against British nationals during the War of 1812. The Republicans' electoral victory did not signal the triumph of the American "asylum of liberty," she argues, so much as demonstrate yet again America's ambivalence about immigration.

Ambitious in scope and thorough in its analysis of a central issue in American history, this book merits a careful reading. To do so is not always easy, for the density of information at times overwhelms the narrative thread. In addition, the weak conclusion, which tries to connect the post-Civil War recognition of citizenship for blacks—the vast majority of whom were native-born—with the preceding discussion of immigration policy is an anomaly in an otherwise strong book. Baseler makes a signal contribution by demonstrating that colonial developments set the terms of America's ongoing debate about its role as a refuge for victims of oppression.

That America was indeed a refuge was certainly the impression of the German emigrants examined by Marianne S. Wokeck in her narrower but equally valuable quantitative study. No other historian has so thoroughly utilized German sources in constructing a portrait of the experience of over 100,000 German emigrants to the British colonies in the eighteenth century. Arguing that this German migration established a model for later population movements to America, Wokeck analyzes the numbers and motives of emigrants (most of whom fled European hardship for American opportunity), establishes the ebbs and flows of their migration patterns, and describes their experiences en route and upon arrival (usually in Pennsylvania). Of particular significance is her examination of the way in which the transportation of German emigrants became a lucrative business for a coterie of London and Philadelphia-based merchants whose profits derived not only from the fares of moderately prosperous passengers but also from selling the labor of less well-off emigrants to eager colonial purchasers. A final chapter offers a similar analysis of eighteenth-century Irish emigrants to the Delaware Valley. Despite some differences—trade links between Ireland and the colonies were long-established, for instance, while those between Rotterdam (the main departure point for Germans) and America had to be created—the Irish experience closely paralleled that of the Germans. In both cases, Wokeck concludes, the commercial organization of the mass movement of opportunity-seeking emigrants prefigured similar migrations to America in the nineteenth and twentieth centuries.

Both books end on an optimistic note. Although Baseler is unsparing in her elucidation of Americans' conflicted approach to immigration, she finds compensation in their adherence to a single standard of citizenship and comparatively generous naturalization policies. Wokeck describes German and Irish immi-

grants being cheated by unscrupulous merchants, robbed by sailors, and exploited by colonial employers, but she argues that such experiences were more than outweighed by the access to opportunity gained by the vast majority of their fellow passengers. Such conclusions surely testify to the durability of America's image as an "asylum for mankind."

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EDWIN G. BURROWS and MIKE WALLACE. *Gotham: A History of New York City to 1898* New York: Oxford University Press. 1999. Pp. xxiv, 1383. \$49.95.

Those who would write a comprehensive narrative history of New York City—or any large city—face a multitude of challenges. How can such a work reconcile the storyteller's need for an authoritative, compelling, and entertaining voice with the contemporary historian's respect for the dignity and the distinctive perspectives of all the city's residents, past and present? Can the narrative provide a sustained interpretation of the city's place in its larger political, economic, environmental, cultural contexts? What are its responsibilities to race, class, gender, the environment? What are the synthesizers' responsibilities to the sources and to the many earlier historians on whom they draw?

This book by Edwin G. Burrows and Mike Wallace represents a heroic effort to respond to these challenges. Its vast quantity of material, chronological order, respect for current and received wisdom, detailed table of contents (including well over 200 carefully phrased sub-chapter titles), well-chosen illustrations and well-designed maps, extensive indexes, and meticulous proofreading make it an exceptionally useful companion to the recent *Encyclopedia of New York City* (1995). Its detailed discussions of European violence against Native Americans, white violence against Africans and African-Americans, male violence against women, nativist oppression of immigrants, and conflicts between workers and employers restore realism to the genteel and school-civics accounts of the 1930s, 1940s, and 1950s and their more recent successors. It describes efforts to enforce the moral economy of the crowd, and it vividly illustrates the shift from violent to bureaucratic enforcement of the hegemony of rulers. It dispenses moral praise and, more often, censure. Burrows and Wallace treat New York City as a theater for the performance of many historical dramas; their treasury of memorable incidents and colorful personalities will long delight writers of feature stories and historians seeking to engage or divert their audiences.

The book contains four parts that might well have been presented as four separate books about cities of vastly different sizes. These four parts employ three distinct approaches. The first 250 pages provide an excellent synthesis of the work of two generations of

historians of New Amsterdam, early New York, and the colonial and revolutionary periods in general. They place the town, whose population grew to about 20,000 in these years, in its imperial and political context, emphasizing the increasingly difficult interactions of New Yorkers and British officials. They focus on New York's relation to the slave-based sugar economies of the Caribbean and other aspects of Atlantic trade but also track population movements, slavery, efforts to establish the Church of England, crowd behavior, and other topics around New York Harbor and the Hudson Valley. This part culminates in detailed accounts of Revolutionary War leaders, political as well as military, who fought within 100 miles of the city.

Two long parts of three hundred fifty pages each bring the city's history to 1843, and then to 1879, employing an ambitious social-history emphasis on colorful individuals and events. The city grew far beyond the size of a large town in these years. Burrows and Wallace find it increasingly difficult to embed a detailed account of the rapidly growing numbers of New York people and events in a sustained, comprehensive account of the city's economic, political, and legal relations with its region, the nation, and the Atlantic world. They make a remarkable effort, discussing topics ranging from the 1790 federal bargain that moved the U.S. capital from New York to Washington, D.C., but secured an integrated national market for the city's merchants, to the chartering of canals and railroads, to the struggle for status. They provide brief introductions to the city's markets for journalism, fiction, art, money, sex, and social status. Their accounts of markets stress the moral failures of speculators and political protests against expensive bread and housing, not explanations of economic growth. They critique the "sunlight and shadow" tradition that starkly contrasts rich and poor in the city, but their extensive, decade-by-decade accounts of the rich—and of their homes and places of amusement—sometimes reinforce that old stereotype. This book's concerns are not with the small producers, the business firms, the mainstream political parties, or the varied Protestant, Catholic, Jewish, and ethnic institutions staffed by so many middle-income and working New Yorkers, and so important to so many both in Greater New York and throughout the United States.

In its last 250 pages, the book offers a more narrowly conceived narrative that features mayoral (and, occasionally, presidential) politics and public works in the city during the 1880s and 1890s, a period in which the metropolitan region's population doubled from about two to four million. Conflicts between Knickerbockers and Yankees, somewhat overdrawn in the middle parts, fade in the face of the city's diversity. The protagonists here are big business, the nouveaux riches, socialists, the new immigrants, upper-middle class reformers, and Tammany Hall. Voters and markets are present but on the sidelines, as are most neighborhoods and suburban towns. Despite its tendency to fall into an old-fashioned account of robber

barons, bosses, and reformers, this section retains much of the variety, color, and comprehensiveness of the book's middle half. But with fewer pages for a much larger population, it cannot provide anything like a detailed treatment of the experiences of those who made up the great city's many social, ethnic, religious, and economic groups.

The book demonstrates the classic virtues of narrative: readability, memorable stories, attention to individual action and character; it has exceptional variety and energy as well. As employed here, however, the long city narrative also privileges origins, early arrivals, the rich, the exceptional, the colorful, the celebrated. It often asks the reader to agree that dramatic events and famous individuals reveal the most significant history. Occasionally, it relies on anachronistic language (environmentalist, p. 837), or on dubious stereotypes (wealthy Catholic trustees as "democratic," p. 752; stock market short-sellers of the moment as destructive "bears" by disposition, p. 847). And it greatly exaggerates the influence of some organizations. The evangelical Protestant Association for Improving the Condition of the Poor, for example, could no more hold "the rich . . . firmly in check" in 1857, forcing them to do "less to succor the poor than ever" (p. 848) than its antislavery counterpart could force all New York merchants to join its campaign.

To their credit, Burrows and Wallace open their discussion of references with a blanket acknowledgment: "A synthesizer, looking back, sees a thief's shadow" (p. 1237). Their approach to documentation is that of journalism. Two decisions that limit the range and vitality of their account call for note here. Determined to present a series of well-told tales, they omit all discussion of the nature and quality of the evidence. This decision deprives their readers of information about the motives, perspectives, and knowledge of the people quoted. It also leaves the reader questioning their accounts of such unlikely phenomena as prices that rise when demand falls. Eschewing footnotes, Burrows and Wallace provide abbreviated lists of sources for each sub-chapter, keyed to a very good, though not comprehensive, list of approximately 2,000 articles and books. Their text provides an illuminating introduction to nineteenth-century historians and other explorers of New York City, but this decision deprives their readers, and other historians, of their assessments of the many twentieth-century historians whose work is the basis of their own. Their decisions to omit evidence and specific references are consistent with their decision to forego a sustained historical argument. They are also consistent with their very fruitful decision to present New York City's history in an extended and colorful narrative.

DAVID C. HAMMACK
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LISA WILSON, *Ye Heart of a Man: The Domestic Life of Men in Colonial New England*. New Haven: Yale University Press, 1999. Pp. xii, 255. \$25.00.

A generation of women's historians has made readers sensitive to the intimate details and everyday experiences of ordinary women's lives. But what of the men they lived with? The emergence of gender history has broadened our focus to men *and* women and made us aware that we know less about the private lives of ordinary men—even in largely patriarchal societies—than we thought. Lisa Wilson has made a significant contribution in analyzing the domestic lives of colonial men: the men in her book speak to us about their worlds, including their feelings about work, their perceived usefulness (or lack thereof) in their occupations, their dilemmas over courtship and marriage, and their anxieties and hopes regarding the responsibilities of adulthood.

Wilson has combed the archives searching for strands of evidence in men's diaries, personal letters, and business correspondence. She quotes extensively from a variety of sources and allows us to hear a diversity of men's voices on a range of topics. Our hearts go out to Ebenezer Baldwin and Sophia Partridge, for example, who could not find ultimately what each wanted in a relationship. Ebenezer hoped to marry Sophia but could not propose before he amassed the resources to support their life together. Sophia tired of waiting and "bagged" him, for Ebenezer, a humiliating turn of events. Even then, break-ups were never simple. Sophia preferred to end the relationship in person, rather than by letter, yet Ebenezer worried that doing so face to face would only serve to disgrace him further. We see the men in Wilson's book as intimate and affectionate, insecure and vulnerable. We learn of the deep emotion felt by colonial fathers, even before the eighteenth-century turn toward sentimentality, through Wilson's moving portrayal of fathers who had lost children at tender ages. Readers will gain a different perspective on the lives lived by white men in colonial New England, who seem more sensitive and fragile than the austere icons of Puritan Fathers.

Wilson's topical approach is both rewarding and frustrating. On the one hand, readers will be satisfied that she has examined so much of the available documentation and put it to good analytical use. On the other hand, this approach fragments stories that readers may prefer whole. When Ebenezer Baldwin, for example, later gives advice to his sister about the suitable qualities in a wife, including the ability to spin shirts, I remembered his earlier failure in the marriage department and wondered what ever became of him. The book is not biographical, yet we are almost fooled into thinking that we can come to know these men by Wilson's deft handling of the material. Wilson's graceful writing and artful storytelling makes us wish we could delve even further into these rarely documented and unfortunately elliptical lives.

Wilson's introduction explains that her analytical framework follows Laurel Thatcher Ulrich in *Good Wives: Image and Reality in the Lives of Women in Northern New England, 1650–1750* (1980). It is unne-

essary and perhaps unfortunate that she contrasts her approach to Judith Butler's in *Gender Trouble: Feminism and the Subversion of Identity* (1990). Wilson writes that "role analysis, rather than the newer idea of gender as a 'performance'—better fits the colonial New England context," and she pleads guilty to being "overly concerned with the 'materiality' of the body or, in layman's terms, with the inherent component of gender not mediated by culture" (p. 4). Readers may be confused because, in fact, Wilson emphasizes exactly the ways in which notions of masculinity were mediated by early American culture. Of course, the materiality of the body was important, and it mattered that those bodies were male. Yet these men were firmly rooted in colonial culture, and so their choices to court particular women or marry without a parent's permission, or be loving fathers, concerned with their children's bodies and souls, were in part "biological," because they were male, but, more important to her study, they were cultural, because these men lived in a specific social world. Wilson may not see herself as "Butlerian," but fortunately her book accomplishes what Butler might prescribe: the sensitive, historical analysis of gender as it was embodied and lived by men and women whose actions were informed and constrained by the social worlds—no less than the bodies—they inhabited.

ELIZABETH REIS
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CYNTHIA A. KIERNER. *Beyond the Household: Women's Place in the Early South, 1700–1835*. Ithaca: Cornell University Press. 1998. Pp. xii, 295. Cloth \$55.00, paper \$18.95.

Historians of southern women have always had to confront the image of the "southern lady" that permeates the field of American history like recalcitrant mildew. No matter how often they assert the existence of real women whose lives were nothing like that of the mythic lady, her shadowy self continues to reappear—despite the repainting provided by countless scholars and their detailed studies of southern women. Cynthia A. Kierner synthesizes much of this recent scholarship to provide a useful and accessible study of real women and their lives in those areas with the largest populations, the most complex societies, many of the oldest settlements, the best-kept historical collections, and the most readily available sources: Virginia and the Carolinas. Her study is not of the early South as a whole, but the extension of her findings into other areas of the region is most likely valid, given that much of what she finds about women's lives beyond the domestic circle can be found in the early North as well.

Kierner's work divides women's lives into discussions of what popular culture told women was proper behavior and descriptions of what women actually did in their lives. She incorporates monographic studies and a considerable number of primary sources that include the lower economic classes as well as elite

white women to argue that white southern women, despite the patriarchal society in which they lived, nevertheless always had a public role of some sort. Their public role, says Kierner, was in a constant state of flux and was renegotiated regularly as expectations about women changed, as domestic needs changed, as political concerns changed, and as society evolved from the colonial frontier of the 1700s to the more urban republican states of the 1830s.

In effect, Kierner argues that the lives of women of the South were like those of women of the North, except when they were not. During the period of the frontier, women worked alongside men, and often in very public places: taverns, printing newspapers, and running businesses. During the Revolution, women voiced their political opinions: wearing homespun, signing petitions, suffering the economic consequences of revolutionary support and destructive warfare in the back yard. During the early years of the republic, women discussed the nature of their role in the republic, embraced the role of republican mother, and petitioned the government for equitable relief from unfair laws regarding marriage and property. After the War of 1812, as society attempted to shunt women off into a controllable separate sphere of domesticity, some resisted and some went along. As the notion that women had separate spheres appropriate to their natures achieved primacy, some women used that argument to employ their moral superiority to reform the public sphere by forming temperance societies and benevolent associations; some women used the argument to assert women's role should encompass only the domestic sphere. But finally, as northern attacks on slavery began to hit home after 1820, the southern patriarchy dedicated itself to ensuring that whatever role southern women played in society, they would do so in a manner that would allow elite white men to maintain their authority over all. The doctrine of separate spheres, so popular North and South, lost out in the South to the maintenance of the patriarchy, and southern women found they could expand their roles only in ways that did not disturb the existing social order.

In all these changes, what made the lives of southern women different from northern women remained the more rural nature of the southern colonies and states. This characteristic, which was true even after the growth of towns in the early nineteenth century, provided northern women with considerably more opportunity to act out in the public sphere, while the isolation of the rural South meant limits on women's activities could be more easily enforced. The most telling limit on southern women's lives, however, remained the institution of slavery, any attacks against which served to increase white men's vested interest in maintaining control over every aspect of life, rhetorically and otherwise.

Southern women are not, in this book, so very different from northern women, and that, for some historians, may be news. Kierner's work illustrates the

constant tension extant between the rhetoric proclaimed by popular culture regarding women's roles and the various ways in which women lived that made clear how divorced that rhetoric was from reality.

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JOHN GILMAN KOLP, *Gentlemen and Freeholders: Electoral Politics in Colonial Virginia*. (Early America: History, Context, Culture.) Baltimore: Johns Hopkins University Press. 1998. Pp. xi, 249. \$46.00.

The question that John Gilman Kolp asks himself in this book is what made the political system of colonial Virginia work on the smallest and most personal levels of the county, the parish, and the neighborhood. Thus, his focus comes to rest on electoral politics: on who ran and on how the enfranchised voted for delegates to Virginia's House of Burgesses in the only election in which they had a voice. Setting aside debates over political stability or instability in seventeenth-century Virginia politics, Kolp turns to the eighteenth century in search of consistent patterns in the choices voters made that illuminate the distinctive political culture Virginia bequeathed to the nation. Thus his work falls into company with some of the most distinguished scholarship on Virginia political history including Robert E. and B. Katherine Brown's *Virginia, 1705-1782: Democracy or Aristocracy?* (1963) and this book's near namesake, Charles Sydnor's *Gentlemen Freeholders: Political Practices in Washington's Virginia* (1952).

Kolp's play on Sydnor's title is significant. Sydnor asked why Virginia property holders voted consistently for their social betters, and he decided that deference between classes provided the glue of colonial politics. Democracy—a word little used in the eighteenth century—according to Sydnor came not in competition over issues for voter interests but in the respect gentry officeholders displayed for the men and families who put them in office. Accepting distinctions between an elite and a larger middling class of freehold planters—and marshaling an impressive array of statistics to demonstrate the superior wealth in land and slaves of candidates over voters—Kolp focuses his attention more on the latter and asks how their choices at the polls reflected social relations.

Recasting issues long at stake in early Virginia historiography reflects a generation of social history scholarship by such notable historians as Edmund S. Morgan (*American Slavery, American Freedom: The Ordeal of Colonial Virginia* [1975]) and Darrett B. and Anita H. Rutman (*A Place in Time: Middlesex County, Virginia, 1650-1750* [1984]), whose work demonstrates that the world of most men and women in the colonial Chesapeake was shaped first and foremost by their neighborhoods. Countering suggestions that in contrast to the stable, cooperative life of New England's covenanted communities, living patterns in Virginia were disorderly and unorganized, recent literature on the Chesapeake has plotted patterns of stable, coher-

ent relationships in the face-to-face interactions of neighbors growing tobacco, raising families, going to church, and attending to the myriad mutual responsibilities of early modern life: giving birth, building roads, sitting on juries, attending militia musters, or serving as constable, church warden, or justice of the peace. Thus it is no surprise that it is in the neighborhood that Kolp finds the key to Virginia politics, and it is here that he makes his most significant finding. Voters supported the men they knew best. "When freeholders gave their votes at the courthouse on election day," Kolp asserts, "they did so in ways that mirrored their place in, and their perspectives on, their small corner of the world" (p. 197).

This world, however, was not everywhere and at all times the same. In describing with great detail the varying political experiences of Virginia's little communities, Kolp makes his second contribution to contemporary scholarship. That Virginians voted for their neighbors does not mean that elections lacked competition or contention. Where rivalries existed among gentry families or counties were fragmented by geography and economic differences, elections for burgesses could be bitterly contested—and, in fact, contested returns constitute one of Kolp's most valuable sources. Here the gentry "experienced political community as contentious, often bitter, heated, and highly competitive" (p. 194). But in counties with a closely linked set of gentry families sharing the privileges of officeholding or with a single family dominating economic and social life, "political community . . . was primarily consensual and noncompetitive" (p. 193). Detailed examination of poll books and contested elections in case-study counties of Accomack, Lancaster, Fairfax, and Halifax leads Kolp to the conclusion that the latter pattern prevailed in Tidewater counties south of the Rappahannock, especially along the James River and, to a lesser extent, across Virginia's Piedmont. Political stability, however, could also characterize the rougher, competitive world of frontier counties when a powerful family prevailed.

Where Kolp's work fits best into the large literature on colonial Virginia politics is precisely where his detailed analysis of elections draws deeply from the broad vein of recent social history scholarship. If this book has a shortcoming, it lies in keeping too much of this scholarship in the background. But at a time of reaction both within and without the academy to social history's provocative concerns for race, gender, and class as the divisions defining American life, Kolp certainly demonstrates how vitally relevant the communities created in the crucible of these divisions were to the political processes that shaped a nation.

WARREN R. HOFSTRA
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RICHARD BROOKHISER, *Alexander Hamilton, American*. New York: Free Press. 1999. Pp. 240. \$25.00.

ARNOLD A. ROGOW. *A Fatal Friendship: Alexander Hamilton and Aaron Burr*. New York: Hill and Wang. 1998. Pp. xiv, 351. \$27.50.

While reaching rather different conclusions on their common topic, these two books complement each other well. Richard Brookhiser's biography, written for an educated public, seeks to enhance Alexander Hamilton's status in the pantheon of America's founders. Arnold A. Rogow's study, crafted for a more scholarly audience, does much to challenge Hamilton's status as it simultaneously tries to rehabilitate that of Aaron Burr.

At the risk of reductionism, Brookhiser argues that Hamilton was "a great man, and a great American. Americans like to think of themselves as self-made, even though few of us are. Hamilton was, and wanted to give others the opportunity to become so" (p. 3). Persuasively, Brookhiser builds a case for seeing Hamilton's incredible life story as an experience he wanted to "generalize" for all Americans. Almost singlehandedly, Hamilton triumphed over obstacles of enormous proportion—illegitimacy, poverty, foreign birth—to become George Washington's trusted advisor, delegate to the Constitutional Convention, creative force behind the *Federalist*, the most prolific journalist among the founders, secretary of the treasury, and among the greatest American constitutional lawyers. Seen in this light—through the eyes of a poor, nine-year-old "orphan" who rose from obscurity to reach the heights of the elite of a foreign country—even the more notorious passages from Hamilton's *Report on Manufactures* (1791), those that praise manufacturing for its ability to provide labor for women and young children, look more like a poorly worded plan to improve the odds for future Hamiltons than the blueprint of a cold-blooded CEO.

Brookhiser's considerable talents as a writer make this a gripping narrative of heroic, albeit tragic, proportions. The sections of the book that deal with New York City and Hamilton's rise to power positively sparkle. Given Hamilton's significant enemies—including Thomas Jefferson, John Adams, James Madison, James Monroe, and Burr—Brookhiser's conclusion that politics was one activity Hamilton "never quite mastered" makes his political successes seem to be nothing less than a virtuoso performance of a Nietzschean will to power.

No biography of Hamilton would be complete without an account of the notorious Weehawken "interview" with Aaron Burr. Brookhiser covers the usual terrain, but specifically rejects as "fanciful" the work of other scholars who note Hamilton and Burr's "interlocking careers and points of resemblance . . . to discern some deep bond of attraction and repulsion between them" (p. 151). And this is precisely the starting point for the second book that establishes a plausible explanation for the root causes of the historic duel.

Although a political scientist, Rogow offers a fasci-

nating and thought-provoking dual biography of Hamilton and Burr that is far more interested in examining their respective character structures than their politics. The result is a detailed, well-documented book that, in addition to making an excellent screenplay for a Merchant-Ivory production, suggests that Burr deserves somewhat more respect, and Hamilton perhaps less, from the nation they both tried to serve.

The similarities and contrasts between Hamilton and Burr, from their births until Hamilton's death at Burr's hand, are extraordinary. They lead the author to "a reasonable assumption," constituting the book's premise, "that the deeper causes of the duel are to be found in the dark recesses of their relationship and in the personal histories that shaped both their characters and that relationship" (p. xi). Rogow builds a complex and complete history of each antagonist, detailing their traumatic childhood years. Questions surrounding Hamilton's paternity and date of birth remain unresolvable; his mother's character and behavior left much to be desired. By an early age, certainly before ten, Hamilton was virtually left to his own talents and abilities to fend for himself. In contrast, Burr was born into the elite of American society but was also parentless by the age of two. From these tragic beginnings, Burr went on to become less than his original station, and Hamilton considerably more.

Rogow painstakingly fills in the essential details, many of them sordid, of each man's public and private life, in order to prepare the reader for a reconsideration of the driving forces behind the duel. Prior to offering his psychological interpretation, Rogow appropriately cautions that, regardless of the documented details of any individual's life, "a clinical diagnosis almost two hundred years later that approximates modern medical science is not possible" (p. 206). With that word of caution in mind, Rogow believes Hamilton suffered from "manic depression, often referred to today as bipolar affective disorder" (p. 206). He points out that suicide is often the conclusion of this particular illness and that Hamilton's duel with Burr was a welcomed cure for Hamilton's sufferings. Given the uncanny similarities to Hamilton's son's death by duel months earlier, this suicide explanation has more than *prima facie* credibility.

Although we will never know exactly what Hamilton said about Burr in February 1804, Burr considered it "derogatory" to his "honor." Like Gore Vidal in his novel *Burr* (1973), Rogow believes that Hamilton's "still more despicable opinion" about Burr had to do with the latter's relationship with "Theo," his daughter. After the death of Burr's wife, Rogow argues that his relationship with his daughter "gradually developed into a relationship closer than almost any known to us between a father and a daughter outside the pages of fiction" (p. 94). Rogow suggests that "perhaps it is permissible to speculate, but no more than speculate," that Hamilton perceived that Burr's relationship with his daughter "had incestuous overtones" (pp.

192–93). While this could explain Burr's behavior surrounding the duel, about which he had regrets later in life, it does not explain Hamilton's clear, lengthy, and unrelenting obsession with Burr. Starting with a "perceptive question" first raised by Douglass Adair in 1955, Rogow suggests that Hamilton and Burr were "distorted" "mirror images" of each other. "Through the process of projection," Rogow writes, "Hamilton was enabled to see in Burr those illicit and inappropriate fantasies, desires, and impulses of his own which he did not wish to confront" (p. 264). Pushing these considerations to their psychodynamic conclusions, Rogow "speculates" that Hamilton's "positive identification with Burr" reached down into his psyche, touching "underlying homoerotic elements and . . . a concomitant and compelling need to defend against an attraction . . . experienced as unacceptable in terms of prevailing social and introjected models of masculinity" (p. 266). While these "speculations" may trouble some historians, it is important to remember that Rogow makes no claim to a "definitive" interpretation of events, even though he hopes that his interpretation is "believable, plausible, and . . . credible truth" (p. xii). Rogow's interpretation is plausible; open-minded readers can determine its veracity for themselves.

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ANYA JABOUR, *Marriage in the Early Republic: Elizabeth and William Wirt and the Companionate Ideal*. (Gender Relations in the American Experience.) Baltimore: Johns Hopkins University Press. 1998. Pp. ix, 217. \$42.00.

Anya Jabour has drawn on a large cache of extant letters between members of a prominent southern couple to draw a fascinating portrait of an intense, if sometimes rocky, relationship. A generation of scholarship on the family, on gender ideals in the South, and on the ideology of domesticity as it played out in multiple venues furnishes the context for Jabour's analysis of the Wirt letters.

William Wirt, born in 1772 and the son of Swiss-German immigrants, rose to the top of the country's legal profession, becoming U.S. Attorney General in the administration of James Monroe. Twelve years younger than her husband, Elizabeth Wirt came from a more prosperous family. Indeed, she refused the marriage proposal from the up-and-coming lawyer the first several times it was made. But once she lost the power to say no that came to a young woman during a courtship—in other words, once she accepted—Elizabeth would never again enjoy the same kind of clout, her husband's many protestations of deep affection notwithstanding. A public man of some note, William traveled through the Upper South endlessly in connection with his own legal practice and with the country's business. Elizabeth thus found herself the marriage partner charged with most of the responsibility for home and family. Jabour uses this situation to point to

tensions in the so-called "companionate ideal" of marriage associated with the rise of the modern family as delineated by Lawrence Stone, Carl N. Degler, and others. As women of the middle and upper classes lost more and more of their economic function in the early nineteenth century, she contends, they concomitantly lost much of their capacity to hold their mates to making good on declarations of devotion to wife and family. Hence the negotiation between equals (relative to earlier patterns) that was at the heart of a new style of family life was tricky for these women to achieve.

Although she provides abundant evidence to show that William fell short of treating his wife like a true partner, Jabour's book fails to convince the reader that this is also tantamount to demonstrating the hollowness of the companionate ideal. This is because the Wirts, with their ten surviving children at one point in their married life, seem implausible as exemplars of the modern family, one of whose defining characteristics was its diminishing size. Historical demographers may differ about why the white American family began to shrink, but everyone agrees that the average number of children started to decline in the very decade (1800–1810) in which the Wirts married. Thus William's seeming obliviousness to the toll taken on his wife by constant child-bearing renders him closer to the eighteenth-century norm than to typical nineteenth-century practice. In fact, the book would have benefited from a more rigorous exposition of just what the companionate ideal was and how it related to the birth of the modern family so that the reader might have a clearer sense of the extent to which the Wirts did or did not fit the mold.

Further, in focusing so relentlessly on the issue of the companionate ideal, Jabour fails to exploit the full potential of her remarkable source. For example, we learn in passing that Elizabeth wrote a book about flowers. One wonders how a woman with so much burdensome responsibility managed to find the time to write and to publish, but Jabour is silent about this subject. The reader would also be grateful to learn more about the relationships between the Wirts and their many children.

These criticisms notwithstanding, Jabour has written a solid book, one that partakes of the appeal of a good novel thanks to its chronicle of the life of a marriage. How could William be so clueless about his wife's needs? How could she be so blind to the importance of his work? Tragically, their shared suffering over the loss of children was what brought them together just before he died. As we all know, in these losses, the Wirts were all-too-typical of many other couples in those years when the death of children was inextricably bound up with their birth and upbringing.

GLENN MATTHEWS
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JOHN H. WIGGER, *Taking Heaven by Storm: Methodism and the Rise of Popular Christianity in America*. (Reli-

gion in America Series.) New York: Oxford University Press. 1998. Pp. ix, 269. \$55.00.

The phenomenal growth of Methodism in the early republic constitutes one of the most remarkable chapters in American religious history. Long ignored by professional historians, a recent burst of scholarship from Russell E. Richey, Christine Leigh Heyrman, and Cynthia Lynn Lyerly has made early Methodist history a dynamic area of inquiry. John H. Wigger contributes to this emerging scholarship with his overview of Methodism between 1770 and 1820. Borrowing heavily from his mentor, Nathan O. Hatch, Wigger identifies Methodism as a popular religious movement that grew by "identifying with middling people on the make . . . making use of an efficient system of itinerant and local preachers, class meetings, love feasts, quarterly meetings, and camp meetings; embracing popular religious enthusiasm; creating a variety of new roles for women within the movement; and making Christianity accessible to African Americans, particularly in the Upper South" (p. 5).

Methodism flourished in the postrevolutionary period when ordinary people asserted their independence. Old patterns of deference faded, and the emerging market economy offered opportunities for hard-working people eager for advancement. Methodism appealed most strongly to these ambitious men and women and encouraged individual initiative, self-government, innovation, self-discipline, and a common-sense approach to religious belief. Like Hatch, Wigger recognizes the crucial role the laity played in the church's expansion but focuses on the clergy, particularly on the itinerant preachers who spread Methodism across the length and breadth of the young republic. Itinerants "represented a distinct social class" (p. 48) drawn primarily from the rising artisan and middle classes. Methodists created a flexible and innovative combination of centralized control through a national network of preachers, circuits, book stewards, and presiding elders and decentralized authority based on class meetings, local preachers, love feasts, and active lay leaders. The class meeting "was the basic building block of early American Methodism" (p. 81). Led by laymen and women, often divided by race and gender, class meetings nurtured the faithful, exercised discipline, and allowed members to sing, pray, and testify. Wigger notes that class meetings were far more important to early Methodism than the more celebrated camp meetings. Strict discipline was central to early Methodism and reflected "the most cherished values of ordinary Americans" (p. 102).

The enthusiastic nature of Methodist worship attracted people from the margins of American society, particularly blacks and women, who found greater influence there than in established churches. Early Methodists embraced dreams, visions, miraculous healing, and spirit possession as marks of divine intervention. Wigger suggests that "this quest for the supernatural in everyday life was the most distinctive

characteristic of early American Methodism" (p. 110). Methodism drew large numbers of black converts, but slavery "tore at the heart" (p. 126) of the movement. White preachers reached out to blacks and preached a message that appealed to them, but black preachers and exhorters account for the spread of Methodism in the black community. White Methodists held a wide range of views on slavery, but antislavery sentiments grew after the American Revolution and resulted in the disastrous 1784 attempt to rid the movement of slavery. That failure demonstrated the pitfalls of tying the movement so closely to popular demands.

Wigger acknowledges that women "formed the backbone of the early Methodist movement" (p. 151). Women did much more than join a church defined and administered by men; they often led in establishing churches, exhorted, served as class leaders, and exerted considerable influence on church policy and ritual. If Wigger is correct, then the centrality of women in Methodism demands a different conception of their history than the one he presents. His focus on the clergy obscures the role of women, and when he attempts to identify converts, he focuses only on white men.

By 1830, Methodism claimed 500,000 members, but the church had changed dramatically from its humble origins. Its members prospered, the church exercised considerable political clout, and its clergy sought greater intellectual respectability. Indeed, "no denomination gained as much social, economic, and political ground as the Methodists" (p. 180). The broad outlines of Wigger's interpretation will be familiar to scholars of religious history, but his narrative of early Methodist history is an important addition to the study of antebellum America's largest denomination.

RANDY J. SPARKS
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CARL BENN. *The Iroquois in the War of 1812*. Buffalo: University of Toronto Press. 1998. Pp. xi, 272. Cloth \$50.00, paper \$21.95.

Not many doctoral dissertations are successfully transformed into minutely detailed yet eminently readable books. Carl Benn has succeeded in doing so for the Iroquoian peoples engaged as allies of both the Americans and the British in a war waged along the Canadian border (which both the United States and Britain claim to have won). The veritable losers were the Native peoples on both sides of the international border, although those on the American side lost much more than their Canadian counterparts. The British were never able to deliver on their vague promise to support a Native homeland, an independent buffer state in the Michigan region. The American victories in southwestern Upper Canada in late 1813, notably the battle of Moraviantown where Tecumseh was killed, shattered the dream of an independent aboriginal homeland. The British failed to press the issue at the negotiations in Ghent because they became preoc-

cupied with the threat posed in Europe by Napoleon Bonaparte.

Benn's in-depth study replaces the earlier works of Robert S. Allen, J. Mackay Hitsman, and George F. G. Stanley on the War of 1812 and supplements the diplomatic history of Colin G. Calloway and Gregory Evans Dowd's examination of the struggle for a united North American Native confederacy. He has mined the archival sources and consulted the relevant publications to good effect. However, one is rather puzzled why he did not consult a parallel study, D. Peter MacLeod, *The Canadian Iroquois and the Seven Years' War* (1996).

Benn examines in detail the Iroquois role in this extension of the Napoleonic Wars, during which the Americans sought to exploit the opportunity to detach Canada from the British Empire as well as punish the tribes that had remained loyal to the crown. The contributions of Native communities in terms of manpower, scouting, field tactics and strategy, and intertribal diplomacy are meticulously documented without exaggerating their participation as the decisive factor in particular British or American victories. The stability of Native alliances and the degree of participation was always dependent on the success of the protagonists or their reasonable expectation of victory. Although Native participation was a vital component of both British and American military operations, neither power could rely on unqualified or enthusiastic support. Neither side appears to have fully trusted its Native allies, although only the Americans proposed that both sides should send away their Native contingents.

As was true throughout the earlier French period, the Iroquois and western tribes fought on their own terms, for their own objectives, according to their own initiative. This resulted in military observations that they were "unreliable allies." At Queenston Heights, for example, they helped dislodge the Americans from their dominant position. Nevertheless, Benn balances their effectiveness by considering their high rate of desertion and their limited scouting activity. He demonstrates that the Americans were more prone to downplay the role of their Native allies than were the British and that during the first three years of hostilities, they failed to learn such essential lessons as the effective use of the militia and proper firing techniques for light infantry. The battle of Chippawa, where the Grand River people suffered heavy casualties, has been hailed by several historians as the turning point in the professionalization of the American army.

Four further characteristics of Native participation are noted by Benn. First is the existence of factionalism, which had long been characteristic of the League of the Iroquois. In both the Grand River and New York Iroquois communities there was considerable division as to the degree of support to give each government. The divided loyalties of the Akwesasene Iroquois emerged in the battle of Crysler's Farm and along the upper St. Lawrence front, as some fought on

the American side and others on the British side. Second, there was frequent communication between the two aforementioned communities, although these consultations never resulted in a united stand. This was similar to the behavior observed in the earlier conflicts of 1744–1748 and 1755–1760. Third, the internal struggle for influence at Grand River between William Claus and John Norton was an extension of the power play between the army and the Indian Department. Fourth, Benn underscores the influence of poor harvests, inclement weather, and insufficient supplies and presents on Native commitment to warfare and resorting to looting and plundering. Maple sugar production and corn planting could justifiably take precedence over fighting. An interesting detail of the 1814 campaign was that the food shortage in Upper Canada was met in part by food supplies from New England farms smuggled through Akwesasne!

A fifth characteristic, which Benn does not develop, is the degree of continuity in Iroquois behavior since the Treaty of Montreal in 1701. The French had been perceived as the "protectors" of Native interests in the Ohio valley and Great Lakes region, and this had resulted in the rather loose Three Fires Confederacy that was in contact with the Seven Nations of Canada. The British assumed this role after the cession of 1763. George Washington, for example, was typical of the Anglo-American colonial speculators who dismissed the Royal Proclamation of 1763 and Indian Department regulations that stood in the way of profit from the acquisition of aboriginal land. The Iroquois, caught between two forces, found neutrality a difficult line to tread.

There are two good maps at the beginning of the book, but several maps dispersed throughout the text to illustrate specific campaigns would have been more helpful. Also, although the events are treated chronologically, the specific dates of events are rarely provided. The endnotes illuminate the text as much as they confirm the author's interpretation. The index is adequate, and the chapters are arranged in a manner that facilitates consultation of specific periods or events. This is not just a good historical study; it is an indispensable work for a proper understanding of the War of 1812, a conflict sometimes presented as the last stage in the "warrior image" of the Iroquois and other aboriginal groups before making way for the "vanishing Red Man" image of the nineteenth century.

CORNELIUS J. JAENEN
University of Ottawa

KEITH R. WIDDER. *Battle for the Soul: Métis Children Encounter Evangelical Protestants at Mackinaw Mission, 1823–1837*. East Lansing: Michigan State University Press. 1999. Pp. xxiv, 254. \$24.95.

In this book, his revised doctoral dissertation, Keith R. Widder has carefully researched the history of William and Amanda Ferry's evangelical Protestant mission on Mackinac Island, Michigan Territory, in the 1820s and

1830s, a time of critical change as the fur trade declined and American settlement expanded in the region. The book organizes and presents much fresh and useful information. Its five chapters explore the origins of the families around Mackinac (chapter one), the missionaries and their outlooks (chapter two), the history of Mackinac 1815–1837 and the responses of its multi-ethnic community to the mission and to Americanization (chapters three and four), and the nature and consequences of the mission experience for the children who lived at its school (chapter five).

For this reader, however, a distracting problem runs through the book. The prologue, entitled “Foreshadows,” begins “The story of the Mackinac Mission shows how the Metis functioned as a distinct group of people after the War of 1812” (p. xxi). Similarly, chapter one (entitled “The Metis Family: Origins and Characteristics”) opens by citing William Warren, author of *History of the Ojibway People* (1885) as capturing “the genesis of his own people, the Metis” (p. 1). Consistent with the use of “Metis” throughout the book, the afterword concludes, “The mistake made by nineteenth-century Americans who viewed Metis and tribal Indians as a single entity must not be repeated. This caused the Americans to miss the Metis as a distinct group of people” (pp. 134–35). The point cannot be missed; the book is positioned as a contribution to Metis history. But were its mission-children subjects Metis, and if so, in what sense?

The preface presents an explanation of terms and names used in the book. “Metis” is not one of those analyzed, however; the category “as a distinct ethnic group” (p. xv) is simply asserted. The reason seems to be that Widder is content to define “Metis” in terms of descent (“blood”) and to assume that descent creates ethnicity. Warren, descended from French and Ojibwa, was therefore “Metis” although he never knew it, never used the term, and was committed to Ojibwa history and traditions. A child such as Me-sai-aince, the daughter of an Ojibwa woman (Widder uses “Chippewa”) and an unknown Canadian father, and brought up by Ojibwa relatives, “illuminates Chippewa religious practices which Metis children could choose to follow” (p. 17). The equating of biology and ethnic identity creates the paradox that children brought up entirely by Indian relatives are inserted into another ethnic category they never heard of. A further consequence is that the Indian sides of these families, often so critical and formative, are silenced and overlooked rather than being taken seriously as basic sources of identity. “Metis” itself, a variable and often problematic social, cultural, and political category, remains unexamined.

Another difficulty is that the sources Widder cites show no sign that missionaries and other writers of the 1820s and 1830s used the term. In effect, his narrative reads the term into texts in which it did not occur (e.g., pp. 4, 54). The categories actually found in the documents of the time are not discussed. Perhaps this is because “halfbreed” or “half-blooded Indian” are not

categories of choice in present discourse. But when we coopt people of the past into categories they themselves and their interlocutors never used, we need at least to be aware of and explicit about the hindsight we are imposing on them and its implications.

In fairness, Widder originally wrote this work in the late 1980s, when Jacqueline Peterson, Sylvia Van Kirk, myself, and others were highlighting aspects of Metis and fur trade history neglected until that decade. He partook of the enthusiasm for seeking out a previously unrecognized group, and he does draw attention to a most interesting population that deserves the attention he has given it. It is disappointing, however, that the book does not engage more deeply with the subtleties and complexities of the group it constructs and the categories it uses. Nor, despite its newness, does it engage with a rich comparative literature on ethnicity that could have fostered more sensitivity to these issues.

JENNIFER S. H. BROWN
University of Winnipeg

ALEXANDRA HARMON. *Indians in the Making: Ethnic Relations and Indian Identities around Puget Sound*. (American Crossroads, number 3.) Berkeley and Los Angeles: University of California Press. 1998. Pp. xii, 393. \$40.00.

American historians generally believe that the cutting-edge scholarship relating to Native Americans focuses on peoples in the eastern part of the United States, characterizing research on American Indians in the West as parochial, local, and regional. As a result, historians often ignore important work conducted on the history of western tribes. It would be unfortunate indeed if historians ignored Alexandra Harmon's excellent book, which provides an examination of several diverse tribes, bands, groups, and villages in a vast portion of the Pacific Northwest surrounding Puget Sound and the Olympic Peninsula. The author deals with Lummi near present-day Bellingham, Washington; Nisqually of the Olympia area; Klallams living near Port Angeles; and numerous others. She surveys a lengthy period of time from the 1820s to the 1970s, tying decades together by concentrating on the impact of white contact on the identity of Native peoples.

Harmon skillfully handles a difficult topic in a sophisticated manner. She began her work as the staff attorney for the Suquamish Indian Tribe in an attempt to settle a boundary dispute. Her legal training regarding real estate and property law served her well as she began her community-based research, but she soon learned that legal matters involving Indian lands had many dimensions. Courts, judges, lawyers, governors, legislators, historians, and business executives have all had a stake in determining who was an Indian and what was an Indian tribe. Harmon's interest in Indian identity and the manner in which Indians and non-Indians identify Native peoples and tribes led her into an extensive research project. Although there is some

discussion about the ways Indians identify themselves, their tribes, and other Indians, most of the work concentrates on how non-Indians constructed Indians and tribes. Harmon demonstrates that this "making" of Indians took many forms over time, largely depending on the motives of those doing the making. This is the heart of the book, and the quest to understand the construction of Indians in western Washington is the *tour de force* of this fine volume.

Before white contact, Indians of the Puget Sound identified themselves by village, often intermarrying and residing in one village and then another. Individuals had power within groups based on family, ability, and spirit acquisition. One's identity—and often one's name—resulted from these powers. British and American traders and settlers identified Indians differently, often categorizing Native people in dualities: good or bad, Christian or non-Christian, Squaxin or Skokomish, Indian or "half-breed." Harmon correctly identifies the treaties of 1854–1855 as pivotal in the discussion of Indian identity and Indian tribes, since the United States forced its policies of treaties and reservations on northwestern Native peoples. Treaties and reservations drew physical and cultural boundaries around Indians, including those who moved to reservations and those who refused. The result of Native decisions to live on or off reservations had monumental implications involving land and identity, since those who chose the reservation often became federally recognized. Recalcitrant, non-reservation Indians often "lost" their designation by whites as Indian because they intermarried with non-Indians and did not live on reservations. The United States and reservation Indians often refused to recognize non-reservation Indian communities.

Harmon's examination of identity includes an informed discussion of the Indian Shaker Church and its unifying nature in terms of identity. She deals with white racism and its impact on identity as well as the importance of mixed marriages and property rights among Native people. She moves in and out of Native communities and Indian reservations using a host of examples from original sources, including government documents, newspapers, magazines, and programs. She ties her narrative to national policies of allotments, reforms, the New Deal, termination, and self-determination. Harmon only peripherally examines Indian sovereignty, except in relationship to Native political organizations and Indian fishing rights. Her final chapter on fishing is superb, as Indians recreated a modern identity through complex issues surrounding fishing rights. This is an important book that will lead other historians to examine Native American identity and the ways in which Natives and non-Natives defined "Indian."

CLIFFORD E. TRAFZER
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HARLOW W. SHEIDLEY, *Sectional Nationalism: Massachusetts Conservative Leaders and the Transformation of America, 1815–1836*. Boston: Northeastern University Press. 1998. Pp. xviii, 283. \$50.00.

The oxymoronic title of this monograph contains an important insight: that Massachusetts's conservative elite after the War of 1812, out of national power and increasingly marginalized by a new popular participatory political culture, expressed their nationalism by waging a political and cultural crusade to regain influence and to have their section's values define the national character. Harlow W. Sheidley's critical analysis of the Federalist-National Republican-Whig leadership exposes in neo-Progressive style the sectional, partisan, and class interests underlying their advocacy of constitutional, unionist, and national principles during this transitional period. Sheidley finds that the elite's effort to regain national power floundered, hampered by a mentality and style of politics rooted in a deferential and patriarchal political culture that was out of step with the increasingly egalitarian and participatory politics organized by mass political parties. Their cultural campaign, however, succeeded to a great extent in putting a New England imprint on the nation's revolutionary heritage and on American identity itself.

The "central focus" of this book is on how the elite "strove to reconcile the opposite impulses of their economic activity and their social ideology" (p. x), because the elite's economic activities after 1815 "were in the forefront of the market revolution that would ultimately undermine traditional values" (p. x-xi). Clearly, the elite struggled as it adapted to the new political landscape, but Sheidley is not as convincing with regard to the economic part of her argument. Of course, economic change helped shape the new political culture, but after a brief description of the market revolution in the first chapter, it largely drops from sight. To argue that the many, often contradictory effects of the transportation, communications, and commercial revolutions simply undermined the elite seems to ignore all the ways in which the elite benefited from these changes.

"Confronted by challenges on so many fronts—national, regional, local, and personal—the Massachusetts conservative elite did not yield to the despair that frequently threatened to overwhelm them" (p. 31). Despair? These were men at the top of their society, wealthy, educated, confident, controlling the labor of thousands who worked—in increasingly miserable conditions—to sustain the elite's more than comfortable lifestyle. Certainly the Massachusetts grandees yearned for influence in the national government, but in my view Sheidley exaggerates the challenges the elite faced at home and minimizes their local hegemony. Massachusetts was Federalist to 1823 and was then a National Republican and Whig stronghold, as Sheidley well knows.

The author's portrait of the elite's political machi-

nations during these transitional years between political party eras culminates in their promotion of Daniel Webster's unsuccessful bid for the presidency in 1836. Sheidley unmaskes Webster's posturing as defender of the constitution and Union as driven by the elite's sectional and class interests. Yet she might have given Webster and his allies more credit for helping to foster positive attitudes among citizens toward the Union and constitution throughout the North, whatever their motives.

It would have been helpful if Sheidley had made some effort to distinguish between Unionism and nationalism, with the latter being a stronger entity than the former. There are other problems, notably a tendency to adopt partisan perspectives as her own, for example in referring to John Quincy Adams at one point exactly as his enemies did: "[a]lways the apostate" (p. 195). Sheidley seems to attribute even Adams's support for internal improvements to elite sectional/economic interest, although Lynn Hudson Parsons's recent biography confirms that Adams's nationalism and willingness to use national power was long standing and often cost him politically.

Much of Sheidley's chapter on Adams's presidency concerns itself with the unfortunate debate that arose in 1828–1829 between Adams and Old Federalists who had been connected with the Hartford Convention. In this "historical controversy," Adams accused the Hartfordites of disunionism, while the Federalists defended the convention as an act of principled patriotism. Sheidley claims this was a "civil war that was sapping New England's strength" (pp. 80–81), but aside from how much ordinary Yankeeedom paid attention to this squabble, can one imagine such a debate taking place in South Carolina or even Virginia, with each side striving to present itself as the more loyally Unionist?

The unrelenting emphasis on the Massachusetts elite's materialism might have been tempered with more attention to their adversaries at both the state and national level, especially Andrew Jackson's promotion of southern and western interests in patronage and internal improvements, for example, as well as his administration's advance of state's rights and protection of slavery. But this book is more valuable in providing a view of internal differences within a group that tends often to be homogenized by historians.

RONALD P. FORMISANO
University of Florida

EDWARD A. PEARSON, editor. *Designs against Charleston: The Trial Record of the Denmark Vesey Slave Conspiracy of 1822*. Chapel Hill: University of North Carolina Press. 1999. Pp. xiii, 387. \$49.95.

When the study of the history of African Americans, and particularly of slavery in the United States, suddenly exploded some thirty years ago, few historians expected a tsunami of publication on the topic a quarter-century later. In more recent years, the emphasis has shifted from the wholesomeness of the slave

community toward greater stress on the devastating impact of the institution on its victims, changes through time, and explicit consideration of gender. In the last decade, also, there have been a remarkable number of studies about slavery in a single and perhaps singular state, South Carolina, where Denmark Vesey literally set up shop in Charleston.

Edward A. Pearson's study of this famous slave conspiracy is unusual in structure. Nearly half is the author-editor's own introduction, which is a nearly book-length essay of description and interpretation. Then there are 120 pages of the transcript of the trials, a dozen more of chronology, another fifteen annotating the conspirators and witnesses, and forty pages of additional documents—mostly letters and selections from newspapers, dating from 1783 to 1861.

The transcript of the trials is a great service, since it makes clear why historians many years ago were fussing at each other about whether the conspiracy was actual or merely supposed. It was in fact very real and quite carefully planned. Vesey and his lieutenants did expect help from country slaves outside the city; and he did aim at killing all whites, except ship captains, and leading a mass migration to Haiti.

The evidentiary problem has been that there are two records of the trials, one published at the time and now rare, and another manuscript version that is now in the South Carolina Department of Archives and History. As editor, Pearson sews the two together with clear indication about his stitching. The result is a newly clear tapestry of an important historical drama. The single most salient disclosure is that the printed version, on which many historians have relied, omits discussion by the conspirators about poisoning the wells of the city. Here, as with most other slave conspiracies, a wall of secrecy clanged down as fast as could be managed at the time.

Pearson's book is especially informative about Vesey's background. He was born a slave, most probably on the Danish Island of St. Thomas in the West Indies, although possibly in Africa. A slaving ship captain took him to St. Domingue, possibly to Africa, and then to Charleston, where he became a carpenter and learned how to read and write. Twenty years later, he purchased his freedom in 1799 after winning a lottery. At the time his conspiracy was discovered, he was an old man in his late fifties.

The author is clear about the multiple intellectual foundations of the conspiracy, which included traditional belief-systems in West Africa; the French Revolution and its reverberations in the Atlantic maritime world, especially in Haiti; Christian texts; and most immediately, the debates about the Missouri Compromise.

Overall, Pearson's interpretation works very well. At times, however, it seems stretched to the straining point by the use of fashionable concepts that have dubious application. The assertion that "Vesey emerges as an agent of cultural revitalization" (p. 128) shuns the fact that the principal result of the plot was

to revitalize the commitment of white South Carolinians to the institution of slavery.

Because this book is so informative, it is unfortunate that some of the peripheral materials are flawed. Presumably both transcripts of the trials are accurate: I lack current access to them. But there are editorial lapses. The "Conspiratorial Chronology" (pp. 283–96) concentrates on Vesey's era and place and includes many other plots and rebellions elsewhere in the U.S. over a period of a century and a half, yet it omits or distorts at least four important rebellions and conspiracies. The index is not reliable and in itself raises the question: why does the historical profession not customarily require that indexers be identified?

These lapses are unfortunate, especially because the author's introduction clearly caps other historical discussions of this slave conspiracy. It recreates Vesey's complex world with precision and makes clear the details and impact of the plan.

WINTHROP D. JORDAN
University of Mississippi

STEVEN WEISENBURGER. *Modern Medea: A Family Story of Slavery and Child-Murder from the Old South*. New York: Hill and Wang, 1998. Pp. xiii, 352. \$25.00.

Scholars of antebellum African-American and abolitionist history have long been aware of the extraordinary case of Margaret Garner, a fugitive slave who, rather than see her four children returned to bondage, murdered one and attempted to kill the others before she was captured. Attention to Garner's story increased after publication of Toni Morrison's *Beloved* (1987), a novel based on the real-life event. But until now, no one has pursued the details and presented a full account of the incident and its implications. Steven Weisenburger has done an excellent job of researching the facts of the case and placing them in the context of the struggle over slavery in the decade before the Civil War.

Not surprisingly, the book is at its best early on, recounting the exciting tale of the Garner family's (Margaret, husband Robert, who lived on a nearby plantation, their four children, and his parents Simon and Mary Garner) January 1856 escape from slavery in Kentucky to freedom across the frozen Ohio River. Even though we know how the story ends, Weisenburger makes us empathize with the Garners, hoping their hazardous escape will succeed. Soon after arriving in Cincinnati, however, they were discovered. Robert Garner tried to defend his family by shooting at a posse of eleven men (including both of their owners and several federal marshals) who had surrounded the house of Margaret's cousin. Margaret Garner, rather than see her children enslaved again, nearly decapitated her two-year-old daughter with a knife and was trying to kill her younger daughter (who died soon after in an unrelated accident) when authorities burst in and restrained her.

At this point, the story naturally becomes less ur-

gent, less compelling. Although Garner did actually testify in court, becoming the only fugitive slave to do so after 1850 (because she was held on a murder charge), the Garners essentially become pawns in a series of legal and political games among various city, state, and federal officials, and the book bogs down a little in the legal machinations.

Weisenburger admits that his goal of telling Margaret Garner's story is ultimately an impossible one: "in a profound sense slaves' ordinary lives are *not tellable*" because they were not free agents (p. 21). Because Margaret Garner, like many other slaves, was essentially voiceless, never writing down her story, having it recounted only by others (usually whites), the reader is thwarted in an attempt to really know Garner, to understand her.

Garner becomes, then, objectified, never speaking, only being spoken about. Rather than struggle against this, though, Weisenburger turns this problem into one of the book's assets, examining the various ways in which Garner was used as a symbol. He devotes nearly an entire chapter to contemporary images of Garner, revealing perhaps less about her own life but much about the society in which she lived. Pro-slavery advocates used Garner's deed to illustrate how blacks were really animals, controlled only through the confines of slavery. Abolitionists made the opposite argument, that slavery drove mothers to ungodly acts, making them crazed. Some took it a step further, martyring the mother who could not bear to see her children made into property.

The book's title, taken from a contemporary painting of the incident, is provocative. The use of the name "Medea" implies a relationship between Garner and her master Archibald Gaines for which there is little actual evidence, but for which Weisenburger makes a good case. Sexual abuse of female slaves by masters and other white men was not uncommon, and Garner's three youngest children were light-skinned, apparently of mixed race. Gaines grieved intensely over the death of the child Mary, crying over her body and taking her home to Kentucky for burial. Scholars may dispute some of the author's other conclusions regarding miscegenation, however. For example, he argues that because census data reveal fewer mulatto slaves in the Deep South, "miscegenation and concubinage and rape were relatively less significant factors in slaves' lives" there than in the border states. Miscegenation was a highly complex psychological phenomenon that southern white men often engaged in but rarely admitted to, especially in the Deep South of the 1850s when slavery was coming under heated outside attack.

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JEANIE ATTIE. *Patriotic Toil: Northern Women and the American Civil War*. Ithaca: Cornell University Press, 1998. Pp. xiii, 294. \$37.50.

Despite a somewhat misleading title, this book makes a singular contribution to the growing body of historical literature on the U.S. Civil War and its effect on gender relations. Jeanie Attie considers the women who participated in the massive soldier relief effort in the northern states during the Civil War, although her book is less about the changes experienced by women themselves and more about the tense relationship between those women and the male leadership of the Union's main relief organization, the United States Sanitary Commission. In this sense, this is not so much a book about "northern women and the Civil War" as it is a study of conflicting notions about gender which were exposed by wartime relief work.

On these terms, however, Attie has produced an extremely thoughtful and compelling book. She begins by chronicling the onslaught of northern relief activities at the beginning of the war and analyzes elite women's initial attempts to organize their work through the vehicle of the Women's Central Relief Association (WCRA). The women soon found their places usurped by liberal, upper-class men who saw an opportunity to promote their vision of a more centralized and enhanced nation state by organizing a more centralized and disciplined relief organization. Led by Frederick Law Olmsted and Unitarian minister Henry Bellows, the United States Sanitary Commission (USSC) thus launched its crusade to systematize and rationalize the relief efforts. Implicitly, the organization rested on what Attie calls nineteenth-century America's "gender compromise" (pp. 11–13): USSC leaders and the female membership accepted the idea of a feminine domestic sphere in which women's work represented voluntary and sentimental contributions to the well-being of their families and communities rather than labor with clear economic value. Granted moral authority over their sphere and over society more generally, nineteenth-century women agreed to relinquish demands for political power.

At the heart of Attie's book is her analysis of how the war, and northern wartime relief efforts, exposed the tensions and falsehoods behind this gender compromise. In this way, Attie ably demonstrates how women invariably calculated the economic realities of their domestic contributions, even as they continued to suffuse their work in sentiment. Even more, she shows how the centralizing nationalism of the USSC men came increasingly into conflict with local women's determination to control their own charitable offerings. Indeed, as Attie observes, rumors of commission corruption and inability to deliver goods into soldiers' hands made women ever more suspicious of the USSC bureaucracy. Thus, says Attie, Union women held ever more firmly to the principle that "charity begins at home," promoting the autonomy of their local aid societies. According to this interpretation, so battered was the USSC by its struggles with women's defense of their local charitable domain that it relinquished plans for nationalized relief for Union veterans in favor of women's voluntary labors on their behalf. In this way,

Attie revises earlier scholarship that suggested a more complete repudiation of women's benevolent efforts in the post-Civil War era.

Attie is less successful, however, at analyzing the war's transformative effects on those women who are, aside from USSC leaders, the subjects of her study. She offers some insight into the experience of Louisa Lee Schuyler, who, as head of the WCRA, served as the principal intermediary between the local relief workers and the male leadership of the commission. But while Attie is clearly interested in women's changing political identities and in exploring links to the postwar suffrage movement, her book provides only speculative suggestions about how the struggles over relief work may have affected the ways in which the female rank and file defined their place in the postwar world. Moreover, since many women chose to work through other organizations—like the Christian Commission or local and state societies—Attie's focus on the women in the USSC orbit can only illuminate the attitudes of a relatively small sector of the northern female homefront.

Nonetheless, as Attie makes clear, the USSC work did represent a bold and important new direction in wartime nationalism. In this regard, despite her inability to deliver a more in-depth analysis of "northern women and the Civil War," Attie's perceptive examination of gender conflicts in the commission offers considerable insight into the precarious state of gender relations, and its relation to the expansion of federal power, in the nineteenth-century U.S.

NINA SILBER

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WARREN B. ARMSTRONG. *For Courageous Fighting and Confident Dying: Union Chaplains in the Civil War*. (Modern War Studies.) Lawrence: University Press of Kansas. 1998. Pp. xii, 171. \$24.95.

This useful, compact study—an updated doctoral dissertation begun more than thirty-five years ago—interweaves military and religious history. Its topics include the authorization, organization, and administration of the Union chaplaincy; the chaplains' duties, practices, and relations with the troops; their interaction with freedmen; their attitudes about the cause and meaning of the Civil War; and, finally, the quality of their contributions.

Several of William B. Armstrong's findings should be noted. For example, the legislative act establishing the chaplaincy announced that its purpose was to provide for the "better organization of the military" (p. 3), suggesting that the government's intentions were secular as well as spiritual. The chaplains were at first ordered to be ordained ministers of "some Christian denomination," indicating the limits of the government's tolerance at that time, or perhaps simply its inattention to religious pluralism. After a rabbi appealed to the president, Abraham Lincoln promised he would urge Congress to include "the Israelites" as

chaplains, and it did in 1862, substituting the words "religious denomination" for "Christian denomination." In the same bill, however, Congress also commanded closer inspection of the clergymen's denominational credentials, apparently in an effort to offer only organized religion to the men. The army, however, had previously ordered that "the wishes and wants of the soldiers" would be given "their full and due weight" in selecting chaplains (p. 6), which suggests that religious democracy was as important as religious denomination in the selection of chaplains. Armstrong finds that, in sum, nearly all the 2,300 chaplains were Christian, and the Christian chaplain's denomination was usually that of the majority of men in the regiment. This reviewer finds one more unusual and revealing fact about chaplain selection: one regiment, the First Wisconsin Heavy Artillery, unanimously chose as its chaplain a woman, Ella Gibson, an ordained minister of the Religio-Philosophical Society of Saint Charles, Illinois. When the appointment came to him for approval, Lincoln told Secretary of War Edwin M. Stanton that he had no legal authority to do anything about "such a question," but he did not object to her appointment. He left the decision to Stanton, and Stanton decided against her acceptance.

Armstrong has used a highly instructive text for his study, the Reverend William Young Brown's manual, *The Army Chaplain* (1863). His other vital documents are the chaplains' memoirs and official reports, which summarized the soldiers' conduct and concerns. These also contained the details of the chaplains' spiritual duties: counseling soldiers who were dying, writing letters home to their survivors, acting generally as morale officer to the men, being "a friend to every man," as Chaplain William Eastman put it in his memoir (p. 125). It is also in such documents that chaplains testified as to the bravery and compassion of the ex-slaves who had become Union soldiers, provided accounts of their efforts to educate the freedmen, and, "almost universally" (p. 95), told of their conviction that slavery was the root cause of the war. Believing that slavery and Christianity were "totally antithetical" is what set Union chaplains apart from Confederate ones, even if they prayed to the same God. It is also this conviction, says Armstrong, in a lengthy digression, that sets the Union chaplains apart from the "revisionist and counterrevisionist" Civil War historians.

One can question the author's apparent willingness to accept the chaplains' words at face value in this and other instances. He declares that their contributions were manifold and beneficial, but this respectful conclusion does not require that the chaplains' judgments and observations should be presented uninterpreted. They were after all, religious witnesses, not simply eyewitnesses.

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DENNIS J. RINGLE. *Life in Mr. Lincoln's Navy*. Annapolis, MD.: Naval Institute Press. 1998. Pp. xvi, 202. \$32.95.

It has now been over fifty years since Bell Irvin Wiley laid the groundwork for the study of the Civil War soldier in his masterful work, *The Life of Johnny Reb: The Common Soldier of the Confederacy* (1943). When Wiley completed *The Life of Billy Yank* (1952), these two works offered historians a solid foundation to explore further the social side of the Civil War. Since the publication of these two books, many articles and studies have extended our knowledge of the wartime experiences of the soldiers on both sides of the conflict. Yet, despite the tremendous amount of literature available, similar scholarship on the U.S. Navy has been lacking. Dennis J. Ringle has written a long overdue study that strives to correct this shortcoming.

Ringle arranged his book topically and begins by examining the important antebellum social conditions and reform movements that affected the navy. He follows this with a discussion of the navy's recruiting difficulties and the use of African Americans during the war. The latter topic bears special mention because the navy, despite lingering prejudice, offered opportunities to blacks. Although these men faced discrimination, they received the same clothing, food, and pay, lived in the same quarters, and fought and died alongside their shipmates. They won a share of equality and upward mobility not necessarily available to them ashore. Making up twenty percent of the manpower of the navy, they clearly made an important contribution.

Ringle's chapters also examine shipboard routine, discipline, medical practices, entertainment, and the men's battle experiences. Ringle, a retired naval engineering officer, is at his best when describing the living conditions on board and the activity in the engineering spaces. He brings his personal insight into the discussion of this subject and brings it to life. Duty in the engine spaces was probably the most hazardous and the least comfortable. Boiler explosions, broken steam piping, and other accidents injured and killed many. Poor ventilation could cause temperatures to rise to 140 degrees in the engine room and over 150 degrees in the fire room. Additionally, the monotonous duty and tedious shipboard routine drove many to despair. Among the sailors, the subject of food was frequently a topic of discussion and complaint. Navy food was, according to Ringle, better overall and healthier than that issued in the army.

Ringle's scrutiny of medical practices is important to our understanding of the Civil War at sea. Dysentery, smallpox, and malaria killed more men than the enemy. While disease killed eight percent of the Union soldiers, it claimed only two percent of the sailors. Ringle asserts that the navy's medical department succeeded because it was better prepared to handle diseases and actually improved the men's medical care during the war. Ringle found that the experience of the

common sailor in battle—also an important and much overlooked aspect of the war—differed widely. While the sailors on the ironclads may have operated in greatly confined and brutally hot spaces with relative safety, crews on wooden ships usually lived in more comfortable conditions but often suffered tremendous battle-related casualties.

Ringle claims that his book is only the “tip of the iceberg” (p. xvi) considering the wealth of sources available on this topic. Nevertheless, he might have made a larger contribution. Ringle focuses on the blockade and all but ignores the western river sailors. He does provide some valuable insights but further comparisons between the different theaters of the war and with the army would have given the study a more analytical aspect, which unfortunately it lacks. The author only sampled a small number of the available memoirs, manuscript collections, and other primary source material and viewed few contemporary newspapers. The book, however, does provide an engaging overview of the social side of the navy during the Civil War. It is useful and will serve as a starting point for researchers.

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KATHLEEN WATERS SANDER. *The Business of Charity: The Women's Exchange Movement, 1832–1900*. (Women in American History.) Urbana and Chicago: University of Illinois Press. 1998. Pp. xi, 165. Cloth \$39.95, paper \$16.95.

Short, crisp, upbeat, and straightforward, this book provides a narrative history of the woman's exchange movement, which began in Philadelphia in 1832 as an effort to provide “genteel” women whose economic fortunes had precipitously declined with some anonymous means of earning a living. Elizabeth Stott and a group of well-to-do women living in a fashionable neighborhood only blocks from Independence Hall opened a depository that received decorative sewing items from women who needed income but did not want to sully themselves by working outside the home. Although Kathleen Waters Sander refers to “isolated Depositories” confined to the Northeast before the Civil War, the chapters on this period focus exclusively on the Philadelphia Women's Depository.

After the Civil War, particularly in the wake of the 1876 Centennial Exposition in Philadelphia, where British decorative arts inspired more good-doing ladies, the number of exchanges rose and formed a national movement of seventy-two groups by the 1890s. As women's voluntarism changed, so did the structure and operation of the exchanges. Under the leadership of Candace Wheeler, the New York Exchange for Woman's Work evolved into a place where women from various class backgrounds could learn skills and sell an array of handmade creations, including food, to upscale customers. Working through

national voluntary organizations, including the Women's Christian Temperance Union (WCTU) and Women's Christian Association (later called the YWCA), and local literary and arts societies, and modeled on the highly successful New York Exchange, the movement reached its peak in the 1880s and spread to cities far and wide. A women's exchange became incorporated, for example, into the program of the Boston Women's Educational and Industrial Union. New exchanges added additional services such as employment bureaus, vocational training, boarding houses, and libraries. Most depended on the patronage of wealthy women, although increasingly exchanges made money from interest on investments and subscriptions to members. Consignors included the formerly wealthy as well as middle and working-class women, groupings that Sander wanted to investigate but that remain vague and elusive, partly because of the limitations of available sources. While her discussion of attitudes toward working women needs more grounding in the scholarship on women's labor history and on middle-class women's reform activities, her emphasis on exchange managers as entrepreneurs provides valuable information on the manager's business skills and the influence of large-scale retailing. A few exchanges endure, but most disappeared between the 1950s and the 1980s.

This brief history adds to the literature on women's voluntary and philanthropic organizations, but it never strays from its chronicle, which results in missed opportunities. In the exchanges, the stories of white self-help and uplift, maternalism, ideologies about women and work, beliefs about competition and cooperation all come together, making them rich sites for cultural analysis, but they receive only general introductions here. Readers might wish to use the exchanges to understand the mechanics of preserving existing class lines and relations. Organizers' anxious concern to provide productive work for needy women calls out for critical explorations of meanings of these terms, especially in light of scholarship by Linda Gordon and others that asks questions about the cultural construction of women's dependency and its relationship to work. Sander seems unaware of the whiteness of the movement and the possibilities it provides for understanding how racialized class practices were performed and reproduced. The study would also have benefited from more reference to the history of decorative arts in the United States and more analysis of the typology that exchange organizers created to distinguish their wares from art on the one hand and “dowdy” (p. 89) articles on the other, and to differentiate between deserving workers and others. More use of recent studies on consumerism and homework would also have been helpful. Where Sander sees primarily a positive story of opportunity and benefits to all, other scholars might be more critical about the complicated class meanings and relationships that exchanges represented. Still, the book is a worthwhile

overview of the exchange movement and some of the colorful characters who created and sustained it.

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GILLIAN GILL. *Mary Baker Eddy*. (Radcliffe Biography Series.) Reading, Mass.: Perseus. 1998. Pp. xxxv, 713. \$35.00.

In the preface to this stellar biography of Mary Baker Eddy (1821–1910), Gillian Gill, biographer of Agatha Christie and English translator of French feminist Luce Irigaray, reveals that she has become an admirer of her subject. She has grown, in fact, to have affection for the controversial founder of Christian Science, a religion, Gill says, she would have been hard put to write a sentence about before 1990. She was at that time simply looking for an interesting biographical subject. Upon deciding to write about Eddy, Gill quickly encountered three challenges. The first, common to all biographers of Eddy except Robert Peel, was difficulty in obtaining access to archival material held by the Mother Church of Christian Science in Boston. The second was previously written biographical accounts that have polarized public perceptions of Eddy as either a saintly instrument of God or a self-aggrandizing fake. Ironically, argues Gill, both poles converge on the assumption “that she deserved no personal credit for anything important she did” (p. xxi). The third was the lack of feminist scholarship about Eddy, which Gill attributes to the fact that Eddy did not follow the prescribed script for women who have survived the filtering process of history: suffragist, abolitionist, or temperance worker. Eddy, says Gill, rewrote the female plot.

Gill incorporates the saga of seeking access to archival materials into the text, the footnotes (which are themselves worth the price of the book), the lengthy research note that follows the text, and an appendix on “the essential published source books.” She chronicles her two-year legal negotiations with the Mother Church, which resulted in access that was “frustratingly limited,” although she acknowledges the help she did receive from the Mother Church, including the offices of a researcher who fact-checked the entire manuscript. What she found was enough, in conjunction with other sources like those at the Long-year Museum in Chestnut Hill, Massachusetts, for her to alter standard accounts of Eddy’s life and present her as a complex and gifted woman.

It was the construction of this more complicated interpretation of Eddy that led Gill into ongoing conversation in text and footnotes with previous biographers, particularly the highly polemical account by Georgine Milmine (now attributed in great part to Willa Cather) serialized in *McClure’s Magazine* in 1907 and 1908, published in book form in 1909, and reissued in 1993 by the University of Nebraska Press (at which Gill takes a jab for not being concerned about the accuracy or inaccuracy of the portrayal of Eddy). Gill

is candid about presenting “a defense counsel’s brief” against the accusations that have typically been rendered against Eddy: hysteria, failure as a wife and mother, plagiarism, authoritarianism, self-delusion, quackery, and adversarial relationships with many who had once been close to her. But because she works hard, as well, “to lay out what could be called the prosecution’s case as fairly as possible” (p. xx), she never, in my opinion, becomes Eddy’s hagiographer. She is, rather, a creative speculator who often leaves it to the reader to decide, finally, how to interpret Eddy’s motivations and accomplishments—although it is also clear where Gill stands. She maintains, for example, that Eddy could not have plagiarized from Phineas Parkhurst Quimby because he left almost no coherent writings from which she could have done so, and that the much-maligned first edition (1875) of *Science and Health with a Key to the Scriptures*, Eddy’s oft-revised textbook of Christian Science theology and healing, was widely rejected not because it was incoherent and badly written but because it was so radical.

Highly compelling is Gill’s feminist interpretation of Eddy’s life and work. While there has not been quite so little feminist attention to Eddy as Gill suggests, hers is certainly the first book-length analysis of the significance of gender in Eddy’s own life and in the way she has consequently been understood—or misunderstood. Gill’s primary interest lay in investigating the process by which Eddy became that rare phenomenon: a woman who founded and led a religious movement that has survived more than a century. Gill argues that Eddy had unusual gifts—“for inspirational leadership and religious doctrine, organizational planning and structure, law and finance, propaganda and public relations” (p. xxii)—that she was prevented from exercising in public for many years, talents that in male citizens would have been encouraged and admired.

Gill’s biography greatly amplifies and takes in new directions Peel’s heretofore definitive three-volume work: *Mary Baker Eddy: Years of Discovery* (1966); *Years of Trial* (1971); and *Years of Authority* (1977). Gill’s work does not replace Peel’s, as Gill herself acknowledges, because of his greater access to archival material and his astute reading of it. But what she offers is what she promised: a new understanding of Eddy as “a real woman with strengths and weaknesses, born in a specific time and place, and facing specific challenges and opportunities” (p. xxii).

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JONATHAN ZIMMERMAN. *Distilling Democracy: Alcohol Education in America’s Public Schools, 1880–1925*. Lawrence: University Press of Kansas. 1999. Pp. xvii, 208. \$29.95.

At last, Mary Hanchett Hunt has received the scholarly analysis she deserves. Historians have attributed to Hunt’s campaign for Scientific Temperance Instruction (STI) a range of impressive achievements. Begin-

ning in 1880, with her appointment to head a department of the national Woman's Christian Temperance Union (WCTU), Hunt won some of the most dramatic legislative successes in the history of temperance reform, as every U. S. state by 1901 adopted laws requiring STI in its schools. The implantation of temperance propaganda in every school possibly prepared the cultural ground for the seeds of national prohibition that were to produce such an abundant harvest by 1920. Key to Hunt's legislative triumphs was a strategy of single-issue, non-partisan, pressure-group action, as WCTU women descended upon state capitals with prayers and petitions. This strategy in turn inspired the activists of the Anti-Saloon League, who led the way to the Eighteenth Amendment. Yet despite the widespread respect historians have given to Hunt's campaign, and the publication of her papers in a microfilm edition more than twenty years ago, no book-length account has appeared until now.

Ironically, Jonathan Zimmerman demonstrates how uncertain was Hunt's conquest of the schools. She met opposition from the outset. Local school boards resented orders from their state capital about what to teach and how to teach it. Hunt countered localism by recruiting a locally based legion of WCTU women to address trustees in a language they could not fail to understand. Then laboratory-based physiologists sought to refute the message of Hunt-approved textbooks that alcohol was invariably a poison. Hunt enlisted her own stable of experts to testify in support and used her army of local workers to spread their words. Next, the "Educational Trust"—the superintendents, professors, and university presidents who dominated educational policy during the Progressive era—took issue with the tactics of fear that informed STI. Hunt attempted to outflank the educational elite by appealing to teachers, who, as women, she assumed, would share WCTU values. The teachers, however, retreated from the conflicting pressures of dries and wets into the fortress of professionalism, denying the pedagogical competence of Hunt and her army of lay women. Hunt therefore had to struggle constantly, often unsuccessfully, to ensure that lessons mandated at the state level were actually taught in the schools. Even within the WCTU, Hunt met opposition, as her nonpartisanship was countered by the partisan projects of national president Frances Willard. After Willard's death in 1898, her successors distanced themselves from Hunt's pugnacity, counseling compromise in the face of educators' solid front of antagonism. Within a decade after Hunt's death in 1906, the structure she built had crumbled into dust.

This book, based on extensive archival research as well as wide reading in the historical literatures of reform, medicine, and education, performs a valuable service in telling the story of Hunt and STI. But Zimmerman aims higher. He wants to portray Progressivism as more complex than a scenario of experts versus the people. Hunt and her followers, he points out, employed the rhetorics of *both* democracy and

expertise in pursuing their conservative aim of buttressing individual character to resist the seductions of drink. In addition to complicating Progressivism, Zimmerman enters today's debate over schools. Although the STI forces hoped to end controversy, not to provoke it, the result of their efforts was not what they intended. Hunt's movement stimulated public discussion over school policy in local communities across the United States and in so doing undermined attempts by experts to foreclose debate. Because of this, Zimmerman argues, STI represents a "democratic achievement" (p. 147). *How* educational policy is formulated, he believes, is at least as important as *what* policy results.

To reach the largest possible audience, Zimmerman deploys a crisp, lively, and accessible prose. Sometimes this results in oversimplification, as when he presents the complex character of Willard as little more than a product of nineteenth-century evangelicalism. Nevertheless, Zimmerman's thorough research, clear conceptualization, fluent style, and attention to—and respect for—public debate should win his book a wide readership both within and beyond the academic world.

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Huron College

LEONARD WARREN. *Joseph Leidy: The Last Man Who Knew Everything*. New Haven: Yale University Press. 1998. Pp. xvi, 303. \$35.00.

This book by Leonard Warren about Philadelphia naturalist Joseph Leidy (1823–1891) represents a strong marriage of intellectual history and biography. Leidy, a lifelong citizen of Philadelphia and professor of anatomy (1853–1891) and head of the Biology Department (1884–1891) at the University of Pennsylvania, was also a founder of American vertebrate paleontology, parasitology, and protozoology. As Warren explains so well, the breadth of Leidy's curiosity was almost without limit: from anthropology, entomology, helminthology, and botany to protozoology, zoology, geology, mineralogy, comparative anatomy, histology, and pathology. As one of the premier naturalists of the nineteenth century, Leidy as portrayed by Warren was a brilliant but compulsive scientist, one of a half dozen individuals in his day who endeavored to encompass the full breadth and depth of nature's bounty.

It is at the nexus of Leidy's passion for observation and facts that Warren focuses his critical eye. He finds Leidy to be a scientist most comfortable harvesting facts and observations, perhaps the last of a generation of naturalists content to collect, classify, and describe without pausing to probe more fundamental questions of "how" or "why" and without sacrificing structural and taxonomic interests for that of experimental science. This is in marked contrast to contemporaries such as Charles Darwin, Louis Agassiz, Georges Cuvier, Asa Gray, Thomas Huxley, John Fiske, Karl

Rokitsansky, Robert Chambers, Richard Owen, and others who were just as interested in theorizing and conjecture. It is here that Warren begins to answer the question as to why one of the most respected American biologists of the century moved from center stage to relative anonymity. However brilliant, Leidy "was old-fashioned and outmoded, even in his lifetime . . . and missed the transition to modern, experimental science, because change meant uncertainty." Instead, he "veered instinctively to a protected environment, physically, socially, and intellectually" (p. 246).

In some ways, Leidy is an enigma to Warren, for he earned his medical degree at a time (1844) when the spirit of empirical medicine and statistical analysis was changing the face of therapeutics. He became a pivotal leader in American microscopy but worked with self-imposed blinders to avoid controversy or more advanced laboratory research. He had membership in the most prestigious scientific societies in the Western world but avoided becoming a public personality. Leidy provided Darwin, Huxley, and others with information to promote their theories of evolution but distanced himself from the academic battles that consumed his generation. Similarly, he was a friend to Claude Bernard and the new generation of German experimental scientists but seemed passive, if not blind, to the consequences of their work. He supported William Pepper's modernization of the University of Pennsylvania medical school but was never a force in moving forward the new interest in experimental science and research.

Leidy was a man who preferred the world of the amateur to that of the full-time professional specialist: he chose the comfort of discovery within the walls of a museum to the challenge of an expedition and willingly and obligingly deferred to European theorists for guidance. Here was a man who lived amid the very forces of change in Western science and medicine and whose books and papers marked the foundation stones for much of the dynamic new science, but who nevertheless arranged to stand "detached" from its theoretical wranglings and internecine feuds and controversies.

Of particular interest to readers is Warren's ability to explain the American scientific establishment within the context of a maturing democracy whose intellectual and cultural expressions reached beyond the traditional boundaries of the New England mind. The egalitarian ethic of Quaker Philadelphia left its own legacy that was as refreshing as it was distinctive. The United States was a nation immensely proud of its scientists, many of whom were earning international reputations and being judged by criteria other than a Puritan ethic. Warren writes a credible history of Leidy and what he came to represent within this larger panorama of American science. To his credit, he writes an honest and insightful story—one that is well worth reading. But the reader is left with a feeling of disappointment, a sense of remorse that Leidy did not use his talents to join in theorizing and to risk contro-

versy in the public debates that followed. Good biographers must learn to accept the hand they are dealt.

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STEVEN CONN, *Museums and American Intellectual Life, 1876–1926*. Chicago: University of Chicago Press, 1998. Pp. viii, 305. \$32.50.

This intelligent, entertaining study by Steven Conn examines the remarkable flowering of museums in the United States between 1876 and 1926, organized by the six categories in which museums sought to institutionalize knowledge: natural history, anthropology, commerce, history, art, and technology. Earlier museums were now characterized as collections "brought together with no purpose" (p. 8). The new museums instead provided wide public access to a "positivist, progressive, and hierarchical" worldview (p. 5). Whether in New York's Natural History Museum or the now-defunct Philadelphia Commercial Museum, physical objects, systematically ordered and displayed, were viewed as "the sites of meaning and knowledge" (p. 15).

The museum craze drew on the passion for order and rationality that also inspired the building of hospitals, libraries, and new universities. It also expressed the late Victorian "fascination with 'stuff'" evidenced in department stores and world's fairs (p. 13). More speculatively, Conn suggests that the United States has always been a visual culture, especially so at the turn of the century as non-English-speaking immigrants flooded the cities. For museum enthusiasts, universities occupied an inferior position, dispensing old knowledge rather than creating new. By the 1920s, the tables were turned. Universities gained a near-monopoly on the creation of knowledge. Museums educated school children, became cathedrals of art, or (as with the Henry Ford Museum and Greenfield Village) served as tourist destinations that professionals viewed as exercises in nostalgia.

A central part of this story is the competition among museums and between museums and universities. In Philadelphia, the struggle pitted the Academy of Natural Sciences against the University of Pennsylvania. In the art world, the competition was between models derived from London's South Kensington Museum (initially realized in the Philadelphia Museum) and from the Louvre (New York's Metropolitan). The outcome was a basic change in the way Americans structured knowledge: object-based epistemology yielded to theory-based knowledge, while certain select objects were revered as "art" (although not a statue of "Rocky" on the steps of Philadelphia's Art Museum, thank you).

Conn denies that museums were created primarily to celebrate and legitimate the wealth of the new rich (Thorstein Veblen's conspicuous consumption). Although some collections were personal monuments

(the Frick Gallery or the Getty Museum), a more nuanced approach reveals that museum thinking itself underwent a revolution after 1870. Nor did museums serve primarily as sites of hegemonic control, existing only to persuade the public that their classifications were the "correct" ones (Antonio Gramsci *cum* Michel Foucault). Foucauldian analysis, Conn argues, ignores the facts that any knowledge requires some framework for understanding, that an object-based epistemology was widely shared within American society, and that museum exhibits spoke to visitors on many levels.

Conn acknowledges that he has not told the full story. All books not about New York require justification, he remarks in one of many felicitous comments. He concedes that the Philadelphia area receives most attention (including the fascinating Mercer Museum in Doylestown), not only because the book began as a study of Philadelphia but because Philadelphians were at the forefront in using museums to organize knowledge. Museum audiences make only "fleeting appearances" (p. 19). The histories of natural science, anthropology, and art are sketched only as they intersect with the museum world, although interestingly so. Sources of financial support are not examined systematically, leaving unanswered the question of why patronage switched so dramatically to universities after 1890.

A self-proclaimed museum buff, Conn is uneasy with his story of rise and decline. He celebrates the democratic aspirations of nineteenth-century museums, laments the sacralization of art, and defends Henry Ford as a latter-day apostle of object-based epistemology (when Ford said "history is bunk," he meant "history as sometimes written," not "history that you can see") (p. 156). But Conn also concedes that advances in knowledge doomed the object-based epistemology and that art museums are true to their materials in that their materials were initially created as objects of worship or decoration. In defense of the older ideal, he notes that the methods of the natural history museums assume new relevance as ecologists inventory habitats by counting, classifying, and collecting. He also concludes that museums draw as large audiences as ever, speculating that objects now function "magically" (p. 262) rather than epistemologically.

This thought-provoking book will be of interest and value to those who create and visit museums as well as to cultural and intellectual historians. Although some of its themes are familiar, it nicely complements and expands the work of Neil Harris and Lawrence Levine and the many museum studies cited. The defense of Ford aside, Conn ably demonstrates that "written history," grounded in theory and diligent archival research, clearly is not bunk.

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DAVID S. CECELSKI and TIMOTHY B. TYSON, editors.
Democracy Betrayed: The Wilmington Race Riot of 1898 and Its Legacy. Foreword by John Hope Franklin.

Chapel Hill: University of North Carolina Press. 1998.
Pp. xvi, 301. Cloth \$45.00, paper \$18.95.

Given the horrific history of the time, it is nearly impossible to pinpoint the nadir of race relations in the post-Reconstruction South, but certainly the Wilmington racial massacre of 1898 comes as close as any single event to marking this moment. Part of a white supremacy crusade that swept through the American South at the turn of the century, the significance of the violence in Wilmington reached far beyond the borders of North Carolina. The white attack on African-American political power in Wilmington stemmed from the same impulse that led American military forces to crush the Filipino insurrection following the war with Spain. The white response to people of color taking control of their political destiny in both instances was a ruthless assault on democracy.

Because what happened in Wilmington cuts so sharply against the grain of how most Americans think about their past, there is little to commemorate the African Americans who were killed and driven out by the white revolt. With the centennial of the race riot in 1998, however, blacks and whites joined in a citywide campaign to recall Wilmington's tragic past and to move toward racial reconciliation. David S. Cecelski and Timothy B. Tyson contribute to this essential venture in what Toni Morrison, in *Beloved* (1987), has called "rememory," bringing together an impressive lineup of historians to examine the Wilmington riot and its aftermath. Seeking to "avoid the temptations of narrow scholarly debate" (p. xiii), Cecelski and Tyson set out to produce a collection of essays that addresses a larger audience. Although working within a genre not known for its wide appeal, Cecelski and Tyson are surprisingly successful in achieving their goal, and they should be praised for the effort. All too few academic historians these days are willing to discuss the larger issues raised by their work and to adopt a more accessible language so that non-scholars do not feel intimidated by or excluded from this conversation. It is hard to imagine a better topic for such a dialogue than with the history of racial violence in the United States and its impact on race relations today.

One of the strengths of this book is its insistence on placing the events in Wilmington in their larger historical and cultural context, so that readers understand not only the crucial facts of the racial massacre but their significance from a variety of perspectives. H. Leon Prather Sr.'s lucid overview, distilled from his book-length study, provides a useful beginning for those unfamiliar with the details of the 1898 white uprising, sending a clear signal to non-specialists that they are, indeed, welcome to participate in the discussion. Following Prather's more conventional narrative are a series of innovative and compelling essays exploring, among other topics, earlier traditions of African-American political activism; the volatile interplay of gender, race, and sex in the South; the role of historical memory in shaping what we know about the

racial massacre; the social order that arose in its aftermath; the riot's portrayal in African-American fiction; and its legacy for the modern civil rights movement.

Out of this fascinating medley of approaches at least two main themes emerge. Perhaps most central is the contention that what happened in Wilmington in 1898 was not an aberration but rather, in William H. Chafe's words, part of a widespread "betrayal of democracy embodied in the denial of citizenship to the descendants of slaves" (p. 279). Steven Kantrowitz perceptively describes the dynamics of this betrayal as "the two faces of domination," a paradoxical history in which paternalism and violence are inextricably linked. As Kantrowitz notes, the intertwining of brutal repression and self-restraint lay at the heart of slaveholding and found its post-Reconstruction expression in the days after the Democratic seizure of power in Wilmington: "The violence necessary to overthrow the elected government had to be followed immediately by an appeal for social peace and harmony, an opportunity for both defeated opponents and potentially hostile outsiders to draw back from confrontation with white supremacist terror and accept the new order of things" (p. 107). The spirit of civility and racial moderation that characterized North Carolina politics in the wake of the Wilmington coup d'état measurably improved the state's image in the eyes of the outside world, but few North Carolinians doubted the reality of the iron fist contained within the velvet glove, least of all African Americans.

Another major theme that threads its way through many of the essays is the critical role of gender in shaping race relations in the South. Glenda E. Gilmore focuses on how white Democrats fabricated stories of outrages against white womanhood in order to motivate white men to commit mass murder, while LeeAnn Whites analyzes the rhetoric of Rebecca Latimer Felton, a leading woman's rights activist and reformer in Georgia who called on white men to protect white womanhood by lynching black men. Laura F. Edwards takes a different tack, emphasizing the agency of African-American women in the struggle for political and legal rights that led to the 1898 confrontation.

Cecelski and Richard Yarborough also examine blacks as historical actors rather than simply victims of white oppression, demonstrating how notions of manhood operated as a force for individual affirmation and political militancy in the African-American community. Whether Abraham Galloway, the black state senator from North Carolina who wore a pistol in his belt during Reconstruction, or the fictional character Josh Green, who heroically leads the defense of the black hospital against the white mob in Charles Chesnut's *The Marrow of Tradition* (1901), African-American manhood offered a vital resource for challenging racism.

These are difficult essays to read, not because they are laden with jargon or because they get bogged down in esoteric debates, but because they are painful.

reminding us of how racial hatred and hierarchy have dominated American history and how these old patterns persist, not yet shattered once and for all. The pain in honestly confronting this past is to be expected, however, for as Morrison has written, "Anything dead coming back to life hurts" (p. 35). Anyone hoping to advance the process of racial healing in the United States could do worse than to read these essays and pass on the stories and lessons contained within them.

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WINSTON JAMES. *Holding Aloft the Banner of Ethiopia: Caribbean Radicalism in Early Twentieth-Century America*. New York: Verso, 1998. Pp. x, 406. \$27.00.

Winston James has written what will become an enduring classic in the history of the African diaspora. He argues that the history of black radicalism in the United States in the early twentieth century is very much the history of the Caribbean immigrants who came to America in great numbers. The immigrants were a select group: young, energetic, ambitious, and unhampered by inferiority complexes. Their experience of World War I and their confrontation with Jim Crow made many resolve to struggle against racial injustice in their adoptive homeland.

The focus is on the period between 1900 and the onset of the Great Depression, when antiracist and anti-imperialist resistance grew more insistent in the international arena. Conventional historiography usually divorces these events from the black experience, but more recent scholarship in African and Afro-American studies demonstrates the linkages between the anti-imperialist impulse in Europe and in Africa and countries of the diaspora. Caribbean islanders did not simply view global upheavals from afar but made major transitions themselves as they migrated from one empire to another. The book includes a study of Cuban and Puerto Rican radicals in New York and Florida during the same era. It compares activists who were driven by an implicit consciousness of race with those who made language and national identity paramount. Finally, in an epilogue that will certainly be talked about, James criticizes Harold Cruse's work on the relationship of Caribbean immigrants to radicalism.

This work challenges the inclination in Afro-Americanist historiography to naturalize immigrants of distinction to the U. S. environment without recognizing the significance and impact of their "West Indian" origins. The result has been a marginalization that James's scholarship does much to dispel. Although he has identified many persons of Caribbean ancestry among the African-American elite, this is no mere contributionist history. It tackles the conundrum created by two streams of scholarship that for much of the twentieth century were kept from intersecting by historians' tendency to see their content in mutually isolating terms. Those accustomed to segregating

blacks from the mainstream while remaining blind to differences among them will be surprised by how James recoups heterogeneity.

James argues strongly for giving immigrants their due as leaders in U. S. black communities. He traces their influence on the mainland to the nineteenth century and suggests that, in many instances, Caribbean accomplishments outshone those of native blacks. James does not, however, argue from a position of superiority. He makes clear his awareness of the different historical and demographic circumstances that shaped mainland and Caribbean people of color, respectively, and his disdain for the petty "tribal" divisions that have occasionally marred the mutual relations of these groups. Instead, the author is most interested in recovering the hidden history of Caribbean radicalism in the United States and in exposing some of the prejudices and misconceptions that for so long obscured it. James's research for this project is prodigious. He utilized biography, fiction, poetry, statistics generated by government agencies, primary and secondary historical literature on Caribbean history, interviews, and the black periodical press in the United States in the 1920s and 1930s, among many other sources. His observations on Claude McKay are particularly trenchant and his forthcoming work on McKay will likely surpass previous studies.

The most dramatic contribution to diaspora history made here is the recovery of the Caribbean role in the radical America of the 1910s and 1920s, but there is more. The analyses of Caribbean activists point to a redefinition of radicalism that departs from the implicit Marxism underlying many conventional usages of the term. The book also revives the view of Garveyism as a highly effective worldwide phenomenon, an immensely creative and innovative movement that operated in many areas with modest resources and under intense repression. James is particularly original in his comparison of Anglo-Caribbean radicals in the United States with those from the Spanish Caribbean. Not only has he crossed disciplinary frontiers in treating areas frequently viewed as remote from one another, but he has also shown the power of variant perceptions of race in helping to determine how particular individuals perceived the struggle for justice. Importantly, James reveals the participation of women radicals in the events of the period and includes some useful vignettes of female activists, expanding on newly emerging work on Garveyite women to include women on the conventional Left as well. In sum, this is an enormously rich and rewarding work, lucidly written and accessible, that will be used and debated for some time to come.

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JAMES J. CONNOLLY. *The Triumph of Ethnic Progressivism: Urban Political Culture in Boston, 1900-1925*.

Cambridge: Harvard University Press. 1998. Pp. viii, 260. \$45.00.

Boston, that quintessentially Irish-American city, has contributed more than its share to the lore of urban politics. Yet, as urban historians now recognize, Boston never produced a centralized political machine comparable to those led by Irish-American politicians such as Tammany boss Charles Murphy or Chicago Mayor Richard J. Daley. Boston's James Michael Curley, the "Purple Shamrock" who cast a long, Celtic shadow over the city's landscape, is often, rather inaccurately, styled a boss. Curley, who proudly called himself "the tribal chieftain who led the invading Irish" (p. 194), knew better.

James J. Connolly's book is a significant contribution to the new revisionist urban political history, which seeks to transcend the immigrant working-class machine versus old stock-bourgeois reform dichotomy that has been at the center of interpretations of urban politics. In Connolly's view, not some primordial antagonism between Irish regulars and Yankee reformers but the transformative impact of the new political culture ushered in by the Progressive era created the defining patterns of twentieth-century Boston politics, including the failure to build a centralized machine as well as the sharp ethnic (and later racial) cleavages that still persist.

In this retelling, Boston Progressivism was a protean, insurgent political style that proved as useful to self-defined ethnic outsiders—North End Italians, West End Jews, and the potent but status-hungry South End Irish—as it did to WASP business and professional elites. After 1909, with the passage of the new city charter ending partisan nominations and replacing ward with at-large city council elections, the old-style, party-based politics, which had compelled Irish and Yankee coexistence under the Democratic majority party tent, gave way to the new candidate-driven, publicity-oriented politics that became the twentieth-century norm.

One of the virtues of Connolly's book is a new perspective on John F. Fitzgerald. No longer treated primarily as the peerless Irish tenor of the political hustings or legendary founder of the maternal branch of the Kennedy clan, "Honey Fitz" emerges as a reluctant John the Baptist for the better-remembered Curley. Fitzgerald deftly assumed the mantle of reformer by supporting the work of the 1907-1908 Boston Finance Commission and championing the new charter, which allowed him to win back city hall in 1910. First elected mayor in 1905, Fitzgerald began the process of ethnically inscribing Progressivism as a struggle between the Hibernian "people" and the Brahmin "interests" that Curley, his rival and successor, carried to completion when elected mayor in 1913.

In Connolly's reading, continuity rather than discontinuity characterized Boston politics before and after World War I. The culturally polarizing issues of the 1920s—the Ku Klux Klan, immigration restriction, and

prohibition—provided Curley with the ammunition he needed to rout residual Yankee resistance to his ethnically combative Progressivism with an Irish brogue. Joseph Huthmacher, among others, has argued that ethnic politicians like Curley experimented with a new “urban liberalism” that prefigured, albeit modestly, the New Deal’s more robust welfare state approach. Connolly goes even further by picturing Curley not only as a practitioner of Progressive social reforms but as a creature of the era’s structural reforms, which opened the way for his preferred political style of subordinating party organization and patronage to personal charisma and polarizing ethnic appeals. Curley’s peculiar Progressivism was labeled “inverted APAism” by former mayor Fitzgerald, who himself had opened the door to what today might be called reverse racism.

Having initially downgraded social and demographic explanations of Boston’s early twentieth-century politics, the author ultimately brings them back in by emphasizing the ways in which the middle-class suburban exodus contributed to the triumph of an “ethnic Progressivism” in the interwar years that both reflected and reinforced the cultural and class resentments of the inner-city Irish. By 1943, when South Boston City Councilman Michael Kinsella insisted that the Irish and not Negroes were Boston’s true “minority race,” the ugly script of future battles over school busing was being drafted.

This admirable book is not without weaknesses. The author gives less attention than might be deserved to the political role of the Catholic Church. As James O’Toole has shown in his recent study of William Henry Cardinal O’Connell, if Boston had a machine during the Curley era, its address was the archdiocese, not city hall. Connolly also gives more space to gender politics than would seem warranted by the modest evidence he adduces of its significance in Progressive-era Boston. Yet, all told, this is a provocative and insightful book, well written and well worth reading.

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AMY BRIDGES. *Morning Glories: Municipal Reform in the Southwest*. (Princeton Studies in American Politics: Historical, International, and Comparative Perspectives.) Princeton: Princeton University Press. 1997. Pp. xiv, 244. \$35.00.

A recent topic on the H-Urban e-mail discussion list concerned the scarcity of scholarship on the history of small cities and the difficulties of researching such history. Amy Bridges’s fine book on municipal reform in the American Southwest helps to fill that gap and shows how to craft such a research program.

Bridges is a political scientist who takes history seriously, as evidenced by her previous book, *A City in the Republic: The Origins of Machine Politics in Antebellum New York* (1984). For the book under review,

her disciplinary background turned her attention to the institutions of government and politics. Her historical inclinations led to painstaking research in the archives and newspapers of San Diego, San Jose, Phoenix, Albuquerque, Dallas, Houston, Austin, and San Antonio. With a sensitivity to both pattern and place, she identifies broad trends in municipal government and tests them against the histories of individual cities.

The book’s title refers to the famous observation by New York politico George Washington Plunkitt that municipal reformers are morning glories, whose efforts blossom only briefly before the comeback of professional politicians. In the Southwest, in contrast, Bridges points out that morning glories—the flower—strike deep roots and thrive year after year. So, too, did municipal reformers craft institutional changes that preserved their power over decades rather than single mayoral administrations. As she notes, her book is an exploration of what twentieth-century reform has meant in cities where it flourished. In the Southwest, she writes, “the vigor of the morning glory is so great that other plants are smothered by it . . . Likewise, the growth of municipal reform in the southwest choked out its opponents, replacing parties with nonpartisanship, party politicians with a civic elite, mayors with commissioners and managers, and competition with political monopoly” (p. 3).

Bridges finds three stages in the progress of municipal reform. In the first quarter of the twentieth century, the commercial-civic leaders of small but ambitious southwestern cities enthusiastically adopted “good government” institutions such as city commission and council-manager charters. Voters who were already sold on the economic development agenda of the elites also embraced reforms that promised to support that development with efficient public services. Between 1945 and 1955, a second wave of reformers “refounded” the governments of their now-booming cities. Realizing the importance of writing the rules in their own favor, they fine-tuned institutional arrangements such as nonpartisan and city-wide elections. They organized nonpartisan groups such as the Good Government League in San Antonio or the Charter Government Committee in Phoenix to design public policies, sponsor mayoral and council candidates, and make sure that the city government promoted and supported economic growth with investments in public infrastructure. In the 1970s, however, minority activists, quality-of-life liberals, and the application of the Voting Rights Act to southwestern cities with large Hispanic populations undermined the “public interest” consensus and brought back a more open politics of competing interests and blocs.

This broad pattern has been identified by earlier students of city politics, perhaps first by the contributors to Leonard Goodall, ed., *Urban Politics in the Southwest* (1967), a source that Bridges acknowledges as planting the seed for her own study. She has taken the rough sketches of other scholars and turned them

into a rich and detailed picture of city governance. As the author notes, it is in cities such as Phoenix and Albuquerque that the goals of municipal reform were most fully realized. It is in the Southwest that we can see most clearly the benefits of civic unity and the costs of minority exclusion from governing coalitions. This book has been recognized by awards from both urban historians and scholars of urban politics. It is indeed a model of meticulous research and nuanced analysis within a broad interpretive framework.

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THAD SITTON and JAMES H. CONRAD. *Nameless Towns: Texas Sawmill Communities, 1880–1942*. Austin: University of Texas Press. 1998. Pp. xiii, 257. Cloth \$37.50, paper \$18.95.

The southern lumber industry does not lack for histories, and none are better than Robert S. Maxwell and Robert D. Baker, *Sawdust Empire: The Texas Lumber Industry, 1830–1940* (1983), which provides the context for this book. What Thad Sitton and James H. Conrad offer, however, is an entirely new perspective from the vantage of the company towns that served as operational bases for this ephemeral empire. Using oral history, company records, and journal accounts, the authors reconstruct everyday life in these forgotten communities, each of which emerged, evolved, then disappeared in the wake of the East Texas cut-and-run logging boom. In the process, the authors demonstrate far more conclusively than Maxwell and Baker just how different southern logging was from the better-known examples in the Northeast, Lake States, and the Pacific Coast.

The study is situated in the vast stands of southern pine that once stretched from Virginia to East Texas. Semisubsistence farmers used these woods as a hunting commons and as a free range for hogs and cattle. These customary trespass rights, the authors point out, helped shape the labor force and land-use regimes of the logging era. Beginning with the arrival of railroads in the mid-1870s, the authors describe with clarity and power the bonanza era of steam locomotives and the powerful mill engines that set the pace for an entire community.

Their focus is the way the companies shaped a distinctive multiracial labor force as backwoods farmers eased into new work rhythms and forms of community life in the lumber towns. Rural experience with isolation, clan violence, and irregular sanitation left inhabitants poorly suited for close-quartered mill-town life. Here the egalitarian tradition of the Texas countryside gave way to a hierarchical society where status was dictated by one's standing in the mill, and where the company quarterboss often carried a Texas Ranger commission.

The authors are sensitive to the limits on this power. Job mobility offered individual if not class resistance, and with mixed success company officials confronted

"flash strikes," hard drinking, gambling, fighting, and sabotage as well as the Ku Klux Klan, the formidable Brotherhood of Timber Workers, and "the ultimate Southern bad habit of [revenge] arson" (p. 81). The authors are also sensitive to the variations in mill-town experience, differences that reflected, often as not, the personalities of owners who shaped their empires to fit various economic, religious, paternalistic, or racial proclivities.

Racial tensions, plus the stress of screaming blades and intense work regimes, set the tone for a community culture of violence. Predominantly white, African-American, and Mexican, company towns could also include several European nationalities and an "Indian quarter." Close interethnic and interracial working relationships gave way to strict off-duty segregation, necessitating multiples of schools, churches, hotels, baseball fields, barber shops, swimming pools, and commissaries. How officials held together this volatile work force is one of the intriguing aspects of the book. To their credit, the authors give voice to these multiple perspectives. For children, sawdust and scrap piles, mill ponds, and discarded equipment were a never-ending source of play. For women, the constant rain of soot from the stacks meant an endless round of cleaning.

For each of these towns, the end was inescapable, given that lumbering in the age of vast virgin forests was unabashedly extractive. After a period of fifteen to forty years, the company and its employees moved on, leaving behind a scene of environmental and social devastation and a few bewildered dogs, cats, or goats. One may wonder why two authors would lavish so much attention on a phenomenon that left so little behind, but in fact their book provides enormous insight into an important constituent of the American frontier experience. Although they allude only briefly to other company towns—Appalachian coal towns, New England textile villages, West Coast logging camps—the authors lay a foundation for understanding social dynamics in any extractive industry. As a study in staples production, the book also shows us the underside of America's brilliant age of urban building. The documentation, drawn from several similar community experiences, leads unavoidably to some repetition, but the authors nevertheless spin a compelling tale, sustained by convincing historical empathy, exhaustive research, and sensitivity to broader themes in social history. The book serves its purpose admirably and is a welcome addition to the history of America's logging frontier.

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JOHN C. HENNEN. *The Americanization of West Virginia: Creating a Modern Industrial State, 1916–1925*. Lexington: The University Press of Kentucky. 1996. Pp. xv, 217. \$32.95.

John C. Hennen locates his local study within a national context. "Far from being isolated," Hennen argues, West Virginian elites contributed to the ideological construction and dissemination of industrial Americanism between 1916 and 1925. Hennen's purpose is to deconstruct how "adherents to the hierarchical elements of industrial capitalism attempted . . . with great success, to implant their ideology into the consciousness" of West Virginia (p. 151).

The book moves chronologically through eight chapters, beginning with the mobilization of public opinion during World War I and ending with the triumphant "sanctification of Industrialism Americanism" by 1923–1924 (p. 146). Hennen expands the term "Americanization" from its typical association with English-only initiatives targeted at making immigrants into Anglo-Americans to include campaigns aimed at making native-born workers into obedient laborers and citizens. The book, a winner of the 1995 Appalachian Studies Award, explores how West Virginian coal owners, government officials, and educators adapted techniques for the "engineering of consent," honed by the Committee on Public Information in World War I, to make "loyalty to the state and nation interchangeable with obedience to one's employer" during the postwar period (p. 2, 4).

Throughout 1919–1921, West Virginia unionists mobilized tens of thousands of workers in a series of militant strikes against the "open shop." In the course of these struggles, workers themselves gave voice and meaning to their own interpretation of "American Ideals" (p. 65). As powerful as these contested ideas were, Hennen explains, the book is less concerned with the content of competing ideologies than with how West Virginian elites successfully imposed a concept of patriotism linked to law and order in the service of profit. The book effectively chronicles the relentless and far-reaching efforts of West Virginia's governors, legislature, state police, public schools, media, and business associations to isolate union activists as un-Americans and ensure that educators inculcated the state's "army of young workers" with loyalty to company and country (p. 145).

The book's theoretical framework draws on the concept of cultural hegemony to demonstrate how the ideology of industrial Americanism was transmitted and legitimated by state elites to impose their class domination. Given the book's emphasis on mechanisms of cultural control and the role of elites rather than on the content of ideologies and relations between social groups, the reader gains only a partial understanding of the contest and negotiation that took place as coal owners maneuvered to have their brand of nationalism dominate the region. Although the majority of West Virginians was white and native born, issues of race, gender, and class also need to be pursued in more depth. An example of two local publications hints at the possibilities. During the same period that the *West Virginia Review* promoted the state's image by racializing workers as the "best Anglo-

Saxon blood to be found in America today," the *West Virginia Clubwoman* denounced native-born workers for participating in the 1921 Miners' March and thereby discrediting the state (p. 123). The mobilization of categories of whiteness and manliness in the construction of Industrial Americanism in West Virginia could have benefited from the inclusion of recent scholarship in these areas.

Overall, readers interested in regional and political studies, as well as histories of World War I, labor, and American nationalism, will find the information on West Virginia very useful. Along with a clear and accessible synthesis of well-known narratives of Americanization campaigns and government propaganda efforts, the book draws on primary research for an extensive exploration of local examples.

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YUVAL P. YONAY. *The Struggle over the Soul of Economics: Institutional and Neoclassical Economists in America between the Wars*. Princeton: Princeton University Press. 1998. Pp. xiii, 290.

Yuval P. Yonay had the splendid idea of reexamining the substantial contributions of the institutionalist school of economics, which flourished in the United States from the last years of the nineteenth century into the 1930s and then faded away. Its leaders included the scintillating dissenter Thorstein Veblen (1857–1929), the creatively reforming John R. Commons (1862–1945), and Wesley C. Mitchell (1874–1948), founder of the still active, although not quite influential, National Bureau of Economic Research. One can recognize a survivor, living on into his and through the century's nineties, in the variously celebrated John Kenneth Galbraith, who mediated the acceptance of Keynesianism but remained, despite his demurrers, essentially an institutionalist.

An energetic researcher armed with a sophisticated professional ideology, Yonay has masterfully stage-managed "the struggle over the soul of economics" between the institutional and neoclassical schools. He has ambitiously separated it into struggles over the "meaning of science," "the scope of economics," "reality and theory," "social relevance and values," and "the history of the discipline," as he entitles chapters four through eight. His admittedly "pro-institutionalist narrative" (p. 76) has to record a series of defeats, although he could show that the institutionalists provided a greater thickness and wealth to economic policy. Why did they fall out of action and memory so fast? This leads to a series of other questions centering on Pontius Pilate's.

A sociologist, Yonay is pursuing the path laid down by the historian of science Thomas S. Kuhn in *The Structure of Scientific Revolutions* (1962) but going further in the sense of the "constructivist" theory of his fellow sociologist Bruno Latour. Kuhn's work had

posited his famous paradigms, grand defining conceptions "that for a time provided model problems and solutions to a community of practitioners." His analysis saw new paradigms succeeding old ones without reference to the underlying scientific truth. Thus he recounted how Copernican theory triumphed over Ptolemaic theory without mentioning the fact that it was just as true during the millennium and a half when Ptolemy ruled expert opinion (*Structure*, pp. viii, 152–57). Accused of having made science subject to mob rule, Kuhn revised his book substantially but not to general satisfaction (2d., 1970).

Guided by Latour, Yonay, for his part, qualifies the word "true" with quotation marks and goes on to argue that scientists arrived at the "'true' [theory] . . . only by negotiation, alliances, and rhetoric," thus by way of professional politics. If this permits partisanship and error, Yonay joins in and refuses to "assume that my interpretation is objective and precise" (pp. 19, 28).

Applied to "the struggle," Yonay's formulations only double the epistemological uncertainties. Despite the promises of his chapters four to eight, he has not said *what* it was the institutionalists actually contributed. He prefers to believe that they had corrected or reduced the evils of free enterprise as permitted under neoclassical laws. Licensed by Kuhn and Latour, he stops well short of seeking facts and ascertainable scientific truth to support his position.

Yet the issue between institutionalists and neoclassicals retains its significance, even if the institutionalists have departed the scene. Much of their work has become part of the economic reality in which we live, although their opponents can argue that institutionalist changes have not been effective to the extent that they denied the elegant neoclassical laws of equilibrium, supply-and-demand price determination, and marginal utility. The neoclassicals reason, for example, that minimum wage laws reduce the employers' incentive to hire marginal workers and hence harm those meant to be helped. Yonay cannot present to the reader a structure of institutionalist theory, the institutionalists having piled up facts without organizing them. Most historians of economic thought attribute the disappearance of institutionalist economics to this deficiency. On the reasons for it, again, Yonay has nothing direct to say.

At all events, Yonay has failed to integrate Keynesian theory into his account. Yet Keynesianism claims to offer a counter-structure of theory and, dominating the textbooks, has superseded neoclassical thought as today's orthodoxy. Incidentally overrunning Keynesian theory, Yonay has confused the issue by discovering "mathematical economics" as the dominant successor to both institutionalist and neoclassical economics (pp. 184–95). But mathematics is only a tool, which all the schools of economics have used. John Maynard Keynes was trained in mathematics, and, although he liked to set his intuition above his calculations, his intricate counter-theory expressed itself in such formulations as

the multiplier and inspired the tremendous proliferation of "mathematical economics" to develop it.

About Keynes, also, I question Yonay's statement that he "demolished Say's Law" (p. 10) of the equality of aggregate supply and demand. Actually, Keynes attacked it artfully but then, irreducible neoclassical economist that he remained, swung round and granted the "indubitable [fact] that the income derived in the aggregate by all the elements . . . in productive activity necessarily has a *value* exactly equal to the *value* [Keynes's emphasis] of the output" (*The General Theory* [1936], p. 20); hence demand would have to equal supply. Keynes had restated Say's Law better than its author had ever put it. And, on a mediocre question of fact, Keynes never held Cambridge University's "prestigious chair in political economy" (Yonay's *Struggle* again, p. 11). On Keynes's recommendation, after he refused it for himself, the position went from Arthur C. Pigou, his senior colleague for many years, to Dennis H. Robertson, his old protégé.

DAVID FELIX

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JEFFREY HAYDU. *Making American Industry Safe for Democracy: Comparative Perspectives on the State and Employee Representation in the Era of World War II*. Campaign: University of Illinois Press. 1997. Pp. x, 261.

JOSEPH A. MCCARTIN. *Labor's Great War: The Struggle for Industrial Democracy and the Origins of Modern American Labor Relations, 1912–1921*. Chapel Hill: University of North Carolina Press. 1997. Pp. xvi, 303. Cloth \$49.95, paper \$18.95.

Most scholars would probably concede that World War I was a turning point for American labor relations. The expanding scope of the administrative state propelled the development of new representational forms that would shape the workplace for the remainder of the century. These two books, in large measure, agree about the important new role of the state in giving birth to "modern American labor relations," but projecting into the future just what characteristics the wartime state bequeathed to worker representation elicits divergent responses. For Joseph A. McCartin, the war left a legacy that reached fruition in the New Deal labor relations system. For Jeffrey Haydu, government provided "new support for America's distinctive open shop order" (p. 10), which would thrive in the 1920s and, some would argue, has enjoyed a resurgence in the late twentieth century.

Haydu, a sociologist, develops his thesis through a series of comparative case studies. The first part explores what historians have called American exceptionalism in labor relations by sketching the impact of the war on forms of worker representation in the munitions industries of Germany, England, and the United States. Although all three faced the challenges of war mobilization and labor shortages which in-

creased the bargaining power of workers, the impact of government intrusion into labor relations left varied legacies, depending on the legitimacy and power of the state and the prewar balance of class forces. In Germany, worker radicalism challenged state authority as well as autocratic management. To defuse militancy and restore order, employers cooperated with the Social Democratic Party in a corporatist system that routinized labor-management cooperation under government supervision. In England, worker unrest focused on protecting craft interests rather than threatening political upheaval. Postwar accommodation, then, relied heavily on encouraging employers to bargain privately with national unions to undercut the more radical shop stewards' movement. American workers neither endangered the political order nor had the reservoir of union strength that their European counterparts exploited. American state involvement largely left in place the open-shop system characteristic of the prewar metal trades with a twist: it stimulated the adoption of employee representation plans (ERPs) as an alternative to real collective bargaining. This solution would dominate the 1920s and, in many ways, return in the 1980s.

Part two of Haydu's book provides additional case studies of American state involvement in railroads and two different shipbuilding centers to further test his notions of "exceptionalism." Munitions, shipbuilding and railroads fell under the jurisdiction of different agencies. Moreover, although many of the national unions involved represented workers in all three industries, each had a distinct pattern of prewar labor relations. By holding politics constant, these case studies enable Haydu to examine industrial characteristics and government policies within one country.

What emerges is not just one American exceptionalism but many. In each of the cases, the outcomes resulted from the unique interplay of government policies and prewar patterns of labor relations. Even in the two shipbuilding centers, one agency could foster two different systems, depending on the conditions in a region preceding its involvement. Thus, these case studies diminish somewhat the author's claim that the government's efforts to resolve the wartime labor crisis ushered in a totally new industrial relations order. Instead, it appears that the state's role was to maintain the status quo as much as possible. Even the more novel developments in worker representation, the ERPs, were neither different enough nor widespread enough to carry the weight that Haydu's analysis places on the importance of the state in shaping the postwar order. In his effort to assign increased responsibility to the state for the open-shop labor relations system, he downplays such factors as employer intransigence, market conditions, and characteristics of unions that did so much to establish the balance of class forces that the wartime state inherited.

McCartin's book traverses much of the same ground, but provides a different framework for understanding the significance of wartime experiments in

labor relations. He stresses that the Progressive-era alliance of organized labor and Wilsonian democracy tilled the ideological soil for the state's increased activities on behalf of unions and worker representation. In particular, the U.S. Commission on Industrial Relations and its chair, Frank Walsh, exposed the authoritarian and undemocratic practices of American industries, creating fertile ground for the seeds of "industrial democracy" that took root during the war. Indeed, it was the notion of democratizing industry, not initiating workers' control, that motivated workers and their friends. Walsh then carried that vague notion of industrial democracy into the deliberations of the National War Labor Board and used it to promote new methods of worker representation, enabling the labor movement to grow to significant stature between 1916 and 1918. War agencies legitimized "three things that resonated deeply among working people: their demand for a rule of law in the workplace, their call for a voice in determining the conditions of their work, and their desire to claim their rights as citizens through their labor" (p. 95).

Walsh's vision of industrial democracy, however, confronted substantial barriers. Employers resisted concessions to the closed-shop demands of unions and to real collective bargaining. Thus, while organized labor increased its numbers, the national unions were unable to establish a presence that effectively institutionalized union leaders' input into the terms and conditions of employment. Meanwhile, government agencies acted meekly when they encountered staunch employer opposition to the state's intrusion into the established work processes. This is a point made far more strongly by Haydu as he links wartime workplace practices to the open-shop order of the 1920s, but McCartin is more effective in demonstrating the range of factors that contributed to employer success and agency failure. Both would agree that wartime improvements in wages and working conditions did not establish any consensus on the postwar role of national unions in an effort to democratize industry. Finally, union leaders themselves took little comfort in wartime experiments in worker representation. The shop committees created by government agencies stole authority from national union leaders, who hoped to use their cooperation with the war effort to consolidate their organizations. In the end, company unions, not national collective bargaining, were triumphant.

McCartin, while disappointed in the results, sees much more that is positive in the legacy than Haydu. Taking exception to the notion of a "fall of the house of labor," he asserts that the dramatic postwar struggles to maintain labor's presence were bitter but not conclusive. Even the acquiescence to ERPs was a significant change demonstrating the indelible impression the war left on shop-floor relations. Industrial democracy, despite its relegation to the margins by political reaction, permeated the vocabulary of industrial relations, altered labor's antipathy to government involvement, and spread the benefits of welfare capi-

talism, creating expectations among workers that would eventually inspire collective bargaining. Following some ideas, then, the war for democracy was a dress rehearsal for the New Deal industrial relations system. Certainly, the more immediate outcome and, perhaps, the more enduring legacy, given trends resurgent in the 1980s and 1990s, was the impetus given to employer resolve to thwart collective bargaining and substitute a form of worker representation that was far more amenable to company control. These two outstanding books should stimulate a great deal of discussion among historians as well as current industrial relations practitioners.

KEN FONES-WOLF
West Virginia University

DAVID M. HART. *Forged Consensus: Science, Technology, and Economic Policy in the United States, 1921–1953*. (Princeton Studies in American Politics: Historical, International, and Comparative Perspectives.) Princeton: Princeton University Press. 1998. Pp. xiv, 267. \$39.95.

For nearly two generations, policy analysts in the United States have generally accepted the widely held creation myth that credits Vannevar Bush and his 1945 report to the president, *Science—The Endless Frontier*, with establishing the framework for America's postwar science and technology (S&T) policy. Political scientist David M. Hart challenges the conventional wisdom in this thought-provoking study. As its title suggests, Hart does not believe that modern S&T policy emerged full-blown from a single source or from a single moment in history, nor does he believe (as many observers have been wont to do) that S&T policy has been somehow insulated from the ideas, values, institutions, and interests that have combined to shape other federal policies. Conducting scientific research and development may have been one thing, but setting national policy for it was surely another. Hart's basic argument is that S&T policy has always been part and parcel of American politics and political development as a whole.

Drawing from a wide range of examples spanning the 1920s to 1950s, Hart outlines the complex origins of American S&T policy, demonstrating how it evolved as "a 'hybrid' of competing visions" (p. 3). He groups these rival conceptions of the state into five categories: conservatism, associationalism, reform liberalism, Keynesianism, and the national security state. Although policy entrepreneurs who championed these various positions rarely held science and technology as a high priority, they nevertheless sought to incorporate science and technology in distinct ways to further their overall societal objectives. Hart convincingly shows how the spectrum of ideological principles that had shaped the debates over economic policy also structured the debates over S&T policy. The two were inextricably linked, as he demonstrates through his examinations of several national research and devel-

opment proposals that had gained wide support but that lasted only a short while or which failed altogether. Hart cleverly shows how these otherwise unsuccessful initiatives reflected the constantly competing visions and preferences at play, and how the S&T policy of the mid-1950s and thereafter was an indelible composite of various precedents once trumpeted and/or practiced.

To punctuate his discussion of how the national security state influenced the country's S&T policy during the early years of the Cold War, for example, Hart notes that by 1953 spending for military research and development (R&D) "made up 90 percent of federal R&D spending and over 50 percent of all R&D performed in the United States" (p. 175). Deterrence emerged as the dominant goal, and this objective was built on high technology and had profound implications for federal funding of science. Nevertheless, there remained an ongoing battle over how best to organize and orient the national security state: by focusing on military mobilization or nuclear deterrence?

Hart's deft interweaving of theory and narrative, combined with his novel selection of historical case studies, will make this book of special interest to historians of science and technology, political historians of the United States during the mid-twentieth century, and social scientists concerned with the formulation of orbital public policies like science and technology. For scholars of this topic, it provides a fine corrective to the all-too-popular perspective that S&T policy hatched from a single vision of the state—a vision largely perpetuated by S&T policy makers themselves, who have too often overly influenced historians. In this sense, this book should be read by anyone interested in the history of twentieth-century S&T policy in the United States.

JEFFREY K. STINE
Smithsonian Institution

ROBERT G. SPINNEY. *World War II in Nashville: Transformation of the Homefront*. Knoxville: University of Tennessee Press. 1998. Pp. xv, 209. \$32.00.

Robert G. Spinney continues an ongoing exploration of World War II's effect on the American homefront by a careful examination of Nashville, Tennessee. This mid-sized city, both burdened and privileged by the regional idiosyncrasies of other southern metropolises, according to Spinney, suffered far less than its northern counterparts during the Great Depression because its more diversified economy was less dependent on industrial wage labor. Consequently, no sense of crisis existed in Nashville during the 1930s, although the city's ruling commercial elite cautiously began experimenting with government programs as a tool for securing old goals of city promotion and boosterism. Despite the limited success of some New Deal federal programs used to encourage economic growth while minimizing social change, Nashville's elite and public

opinion at large remained antistatist and skeptical about any government invention, federal or local, in the economy.

It is Spinney's thesis that World War II's greatest impact on Nashville was to change dramatically this antistatist political culture, making both federal spending and local government planning widely acceptable by the 1950s. On the surface, the war seemed to have had little effect on the city. War did bring unprecedented prosperity to Nashville, despite the fact that few defense industries were located there. Between 1940 and 1950, the city's population increased only four percent; the same mayor reigned from 1938 until 1951; and Jim Crow remained normative, as did the city's hostility to organized labor. Unlike during the 1930s, however, public opinion perceived a very imminent crisis in terms of increased juvenile delinquency, public health concerns over rapidly increasing rates of venereal diseases, and inadequate housing and child care facilities. These problems caused Nashvillians for the first time to demand immediate public-sector solutions. Combined with new confidence engendered by the national government's successful conduct of the war, the real revolution was this change in attitude toward the government's role in both the economy and social welfare, which Nashvillians at all levels now enthusiastically embraced.

The postwar years bore witness to this remarkable change in public attitudes in the transformation in municipal government, public policy, and public housing. No single agency better exemplifies this transformation than the Nashville Housing Authority (NHA), according to Spinney. Viewed with suspicion and hostility at its creation in 1938, by the end of the 1940s former enemies had become supporters and the NHA's projects enjoyed broad public support, setting the stage for a remarkable urban renewal project that transformed the central slum district into the revitalized Capitol Hill area between 1949 and 1954.

This Capitol Hill Redevelopment Project was the most notable indication of Nashville's declining antistatist sentiment, according to Spinney, because a broad coalition of politicians, public planners, and the old commercial-civic elite utilized federal urban redevelopment money to revitalize the central city. Spinney also weaves into this larger theme topical consideration of Nashville's African-American and Jewish communities, and a curious chapter on how wartime civilian defense contributed to later comprehensive city planning. He also offers an insightful explanation of the consolidation of Nashville's city government in a lengthy power struggle with surrounding Davidson County after 1945, occasioned by the new awareness on the part of the old commercial-civic elite and the public in general that strong local government was now necessary to achieve new economic and social objectives.

This excellent study is perhaps most important in revealing through the Nashville example both the regional and local diversity of the American homefront

during World War II. By raising so many questions, Spinney's monograph invites further research into other cities and locales to see if indeed the 1940s represent a historical watershed similar to the one he discovered in Nashville.

DURWOOD DUNN

Tennessee Wesleyan College

AMY BENTLEY, *Eating for Victory: Food Rationing and the Politics of Domesticity*. Urbana and Chicago: University of Illinois Press, 1998. Pp. xiii, 238. Cloth \$44.95, paper \$19.95.

As World War II reached its fiftieth anniversaries during the past decade, historians produced a flood of scholarship on the domestic face of war, examining racial and ethnic groups, children, business, sexuality, advertising and propaganda, film, civic obligation, and sacrifice. In her compelling study of food policy and behavior in the United States, Amy Bentley has drawn upon most of these works and voluminous government and other records to elucidate the meanings attached to how American government officials, business elites, and citizens managed the distribution and consumption of food in wartime.

The book is organized around six elements of food activity, with Baltimore serving as the site for in-depth investigation. Bentley looks first at policy, examining Americans' wartime anxieties about food and government efforts to alleviate that worry, principally through price controls to limit inflation and rationing to ensure equitable distribution. Next she focuses on what she calls the "Wartime Homemaker," an ideal fashioned by the government and media to mobilize women's support for rationing. Bentley then dissects the content and ritual of the family meal, idealized in Norman Rockwell's "Freedom from Want" and promulgated in government propaganda that instructed Americans not only about nutrition but also about family, gender, and racial relationships.

A fascinating fourth chapter looks specifically at policy and propaganda surrounding two scarce foods, meat and sugar, explaining how these food products themselves carried gendered meanings: meat as masculine and sugar as feminine. Bentley next investigates victory gardening and canning, noting that in 1943, three out of five households grew gardens, producing more than forty percent of the fresh produce that Americans consumed, and women preserved more than 4 billion jars of food at home and in community canning kitchens. A final chapter examines United States food policy in the immediate postwar years, arguing that the nation could have done much more sooner to alleviate the famine and starvation that spread through many parts of the world.

A number of themes weave throughout the book. Ever present when business and government elites had to make decisions about food policy were two dilemmas: how to mobilize popular support for sacrifice without alarming citizens about the food supply or

diminishing the belief in American abundance, and how to mobilize women to support the war effort, specifically the conservation of food, without substantially altering the traditional gender order? It is not surprising that officials tended to resolve these dilemmas by containing change. Thus for example, they propagated a Wartime Homemaker ideal that expanded somewhat women's presence in the public sphere both symbolically and actually, but they resisted giving women a significant voice in food policy making.

Bentley pays consistent attention to class and race, demonstrating African Americans' energetic participation in community food activities even within the context of exclusion from or segregation in government propaganda and programs. She analyzes the implications of the mass departure of black women from white women's kitchens during the war. Bentley recognizes the class-differential impact of food policy, quoting a low-income person about meat rationing: "We use the same amount of meat, since we didn't use much before" (p. 110–11). She notices that while most propaganda about food portrayed a middle-class ideal, that carrying nutritional instruction was aimed at the working class.

This book also documents the primacy of corporate goals and the power of business interests to thwart women's desires. The food processing and distributing industry, for example, got Congress to defeat mandatory grade labeling of certain food products, and women complained in vain about the discrepancy in sugar allotments that favored commercial bakeries.

The book ends with a rather superfluous epilogue, in which the author seems to strain to connect the wartime situation to food habits today, drawing on such contemporary phenomena as Spam culture, the growth of portion sizes, the absence of rationing during the Persian Gulf War, and ecofeminism. Readers would have been better served had she brought together the conclusions from each of the chapters. Yet even without such a conclusion, this highly analytical and engagingly written book offers the reader a greatly enhanced understanding not only of food policy and behavior during World War II but also of how deeply and variously gender is embedded in American institutions, from food practices to the individualist-communal tensions that run through the nation's history.

SUSAN M. HARTMANN
Ohio State University

DENNIS J. DUNN, *Caught Between Roosevelt and Stalin: America's Ambassadors to Moscow*. Lexington: University Press of Kentucky. 1998. Pp. xii, 349. \$29.95.

The relationship between President Franklin D. Roosevelt and his ambassadors to the Soviet Union followed a familiar pattern of shared views and objectives followed by disillusionment on the part of the ambassadors as they struggled with living in the Soviet Union and dealing with the realities of Stalinism. All

but Joseph E. Davies (i.e. William C. Bullitt, Laurence A. Steinhardt, William H. Stanley, and W. Averell Harriman) shed their initial optimistic expectations and to some degree turned away from Roosevelt's preferences on how to deal with Joseph Stalin from recognition in 1933 through the Yalta Conference in 1945. Dennis J. Dunn has provided a reinterpretation of this relationship, drawing on earlier studies of the ambassadors and introducing a limited number of Soviet documents.

In reviewing the backgrounds and diplomatic service of each American ambassador in Moscow, Dunn has mastered the challenge of connecting each ambassador's experiences in Moscow with the larger narrative of Roosevelt's policies toward the Soviet Union and the flow of events in Asia and Europe. Just when you anticipate that Dunn is going to get bogged down in the Moscow embassy's problems with backed-up toilets, exchange rates, and luggage inspections, Dunn skillfully shifts the narrative to Roosevelt and escalating problems with Japanese expansion, Nazi Germany, and, finally, keeping the Allied alliance together until victory in 1945.

Dunn finds much to praise in the assessments and recommendations submitted by the ambassadors and their professional staff in the Moscow embassy. Dunn understands the ambassadors' initial optimism on dealing with Stalin and endorses their shifts, with the exception of Davies, to a critical stance on Stalin's policies. When the ambassadors and staff—again, Davies is the exception—repeatedly recommended to Washington that it shift to a quid pro quo approach with Stalin and keep him at arms length as an unreliable expansionist tyrant, Dunn applauds their assessments as much more astute than the views and tactics of Roosevelt.

Since Roosevelt has received mixed assessments from most historians on his understanding of the Soviet Union and his negotiating approach toward Stalin, especially during World War II, Dunn's critical perspective on Roosevelt does not really break new ground. Among recent assessments by Robert Dallek, Warren Kimball, and Frederick Marks, Dunn is closest to Marks, although he does not cite his reassessment of Roosevelt as a diplomat. Dunn is clearly influenced by the post-Cold War revival of an emphasis on ideology influencing Stalin's policies and finds little of a positive nature in Roosevelt's policies toward Moscow from recognition in 1933, where Dunn notes the lack of firm agreements on issues of debts and propaganda, to the Yalta Conference, which Dunn depicts as the culmination of such flawed Roosevelt assumptions as the belief that he could personally win Stalin over to wartime and postwar cooperation and his unwillingness to stand firm against Soviet domination of the Baltic states, Poland, and Eastern Europe in general. Dunn's reliance on hindsight, however, sometimes undermines a fair assessment of contemporary perspectives, such as when he berates Roosevelt for worrying about another Nazi-Soviet deal after June

1941, when he suggests that the war in Europe was essentially over before Operation Overlord, and that FDR did not have to negotiate to get Stalin to enter the war against Japan.

Dunn views FDR as having a revised Wilsonian perspective linked to a belief that the Soviet Union and the U.S. were "on convergent paths, where the United States was moving from laissez-faire capitalism to welfare state socialism and the Soviet Union was evolving from totalitarianism to social democracy" (p. 3). Over a dozen references are made throughout the text to this thesis, and in the epilogue Dunn stresses this theme as central to FDR's motivations and flawed strategy. Yet he cites no direct evidence and provides only two indirect sources: a 1981 interview with Harriman and a reference to FDR expressing this view to Secretary of Labor Frances Perkins during the war. Most scholars have given little support to the importance of the convergence idea in FDR's calculations: Dallek does not mention it; Marks mentions it once; and Kimball refers to Sumner Welles's memoir reference to a conversation with FDR. Considering the appeal of the "Soviet experiment" to some liberals in the early 1930s, it is plausible that the convergence thesis drifted through FDR's thoughts, but he expressed significantly more concerns, before and during the war, about how to deal with the emerging challenges of Japan and Adolf Hitler and how to win the war and produce a lasting peace settlement.

THOMAS R. MADDUX
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BRUCE E. FIELD. *Harvest of Dissent: The National Farmers Union and the Early Cold War*. Lawrence: University Press of Kansas. 1998. Pp. x, 244. \$35.00.

In 1902, the Farmers Union emerged from the Texas-based Farmers' Educational and Cooperative Union. By the 1920s, the association had organized farmers across the Midwest. Farmers Union leaders preached the gospel of cooperative buying and selling as well as the improvement of agricultural education and the elimination of speculation on the commodity markets. Most important, the Farmers Union advocated cost of production prices plus a reasonable profit. The advocacy of government intervention in the agricultural economy, often argued with militant fervor, earned the Farmers Union a left-of-center reputation on the political spectrum.

By the end of World War II, the Farmers Union also had gained a reputation as a strong voice for grain farmers who were less affluent and conservative than those who joined the Farm Bureau. The Farmers Union also voiced its collective opinion about foreign policy, particularly between 1945 and 1950: that is, during the early years of the Cold War. In fact, the Farmers Union turned away from its previous emphasis on agricultural policy making intended to increase commodity prices and improve the standard of living

for farm families. Instead, during the late 1940s, the Farmers Union became a major critic of the Truman administration's foreign policy.

Bruce E. Field has written a history of a major agricultural organization that departs from traditional institutional and economic approaches. He argues that the Farmers Union voiced a radical attack on the Truman administration from its national and state offices to the rank and file members in the counties. Field contends that by advocating an anti-expansionist foreign policy and rejecting the argument that the Soviet Union posed the primary danger to world peace, the Farmers Union quickly became identified with communism and, if not an outright supporter of the Soviet system, a fellow traveler in relation to the Communist Party. Instead, Field contends, the Farmers Union advocated world peace through international cooperation directed by the United Nations.

Once the critics of the Farmers Union suggested that the organization sympathized with communism, they created a rift between Fred Stover, Iowa Farmers Union president, and James Patton, national president of the organization. Stover favored no compromise on principle when attacking the Truman administration's foreign policy, while Patton advocated accommodation to keep the Farmers Union well positioned as a player in the complicated business of formulating postwar farm policy. When the Korean War began in June 1950, Patton placed the Farmers Union squarely behind the Truman administration and criticized those who did not support the war effort. This action became a turning point for the Farmers Union because it divided the organization even more over the issue of whether war contributed to the decline of the nation's small-scale farmers by depriving them of agricultural workers who left the land for the military or war-related industrial jobs. The Farmers Union, however, found that it could not easily distance itself from charges of communist sympathy by the Farm Bureau, American Legion, and private individuals. As a result, the Farmers Union became an increasingly marginal voice in political affairs.

Consequently, between 1950 and 1954, Patton and others crafted a plan to improve the image of the Farmers Union and purge the organization of its radical faction. By so doing, Field contends, Patton dramatically altered the immediate postwar foreign policy position of the Farmers Union. Patton now argued that the Farmers Union supported the Korean War because it championed the work of the United Nations. Field claims, however, that the Farmers Union missed its opportunity to provide an alternative direction to the formulation and execution of American foreign policy during the Truman years.

The strength of this study is Field's analysis of an important agricultural organization in relation to American foreign policy during the early Cold War. Whether the Farmers Union could have remained a strong advocate of international cooperation via the United Nations and a supporter of expanded world

trade through reciprocal agreements negotiated by the Truman administration are both moot and problematic arguments. Agricultural historians will be disappointed that Field did not address farm problems and policy suggestions and alternatives in more detail, but he has provided a fresh and innovative look at an important agricultural organization within the context of American diplomatic history from 1945 to the mid-1950s. This book will aid our understanding of diplomatic and agricultural history during the post-World War II period.

R. DOUGLAS HURT
Iowa State University

JEFF LAND. *Active Radio: Pacifica's Brash Experiment*. (Commerce and Mass Culture Series, number 1.) Minneapolis: University of Minnesota Press. 1999. Pp. xiii, 179.

MATTHEW LASAR. *Pacifica Radio: The Rise of an Alternative Network*. (American Subjects.) Philadelphia: Temple University Press. 1999. Pp. xiii, 277. \$34.95.

Arguably more than in any other society, the media in the United States have been central to political discourse and have reflected that country's historical trajectory. Debate on media responsibilities and their associated freedoms are contiguous to the evolution of the American Constitution and the commitment to free speech. At times, however, the media's confidence in their freedom from political interference has been contested, most notoriously during the "reds under the beds" scare that gripped the country after World War II. The political reaction to media that challenged the Cold War consensus reached its zenith during the dark days of the Vietnam War, shortly before Richard M. Nixon, Watergate, the publication of the Pentagon Papers, and the decline of the imperial presidency generated among journalists a renewed self-assurance.

Pedestrian accounts of the American media have conveniently ignored those that provided the basis for a serious challenge to the "establishment" and the political culture that it promoted. Now, at last, that fascinating story unfolds in two welcome books that were published at the same time and that deal with the same subject. Although at first their (coincidental?) simultaneous publication may suggest they are competitors, careful reading discloses that they are in fact complementary analyses. Jeff Land provides a concise introduction to the Pacifica Network, which started in San Francisco as an experiment in pacifist broadcasting after World War II and was later extended to affiliate stations in New York and Los Angeles. Whereas Matthew Lasar's treatment of the station is the result of thorough empirical research that is strong on narrative, Land locates his discussion within a framework that embraces debates on the theoretical foundations of pacifism, liberalism, and communitarianism and draws on the principal exponents of each to lend his arguments the required density. Neither au-

thor is particularly sympathetic to the station's legendary founder, Lewis Hill, who is portrayed as an egoistic figure. Hill's arrogance—feigned or otherwise—is at the heart of Land's volume. His description of Pacifica as a "brash" experiment reinforces what he calls the "swagger" of the station (p. 145), and he argues that such an attitude was central to the station's pacific zeal and its fight against the innate human passion and enthusiasm for war.

Both Lasar and Land agree that Hill's vision provided the energy to motivate the station, a sentiment that is captured succinctly by Land: "Radio in Hill's vision," he writes, "retained its original potential to transform every living room into a genuine public forum and cultural Mecca, a place where, in the wake of Hiroshima and Auschwitz, people would hearken more seriously to a discussion of peace" (p. 38). Hill realized early in his career the power that the media could exercise, echoing the idealism of Guglielmo Marconi, the inventor of radio. Land reminds us, however, that Hill was as disturbed about the destruction by market forces of radio's potential as he was moved by its possible value to the pacifist cause, a familiar theme to readers in an age acquainted with warnings of the "dumbing down" of the media. Land glosses over Hill's suicide in 1957 and appears to suggest that his decision to take his own life can be explained by his depression over serious challenges to his leadership of the station and the overwhelming pressures to transform its approach to programming. Hill was depressed that he had not changed the world, the burden of many a visionary. In contrast, Lasar provides a more rounded picture of Hill. We learn much more about his life, the beginning of his lifelong involvement with pacifism (two very interesting chapters on his time at Coleville, a camp for conscientious objectors), and his severe medical problems: the crippling arthritis, the strokes, and the medication that left him depressed, isolated, self-absorbed, and alone—feelings that only magnified his melancholy and paranoia.

Both authors document in detail the changes that swept the station's programming policies after Hill's premature death, though they disagree on the causes. Lasar explains the changes in terms of the reaction to McCarthyism, which, if true, verifies the station's courage. The magnitude of the Cold War culture that pervaded the U.S. at this time undoubtedly pressured the media to conform or risk being considered subversive. More convincing is the argument defended by Land, who believes the shift in programming was more gradual and was most apparent in the 1960s. Pacifica was central to the anti-Vietnam War agitation and the creation of the counterculture that the period is best remembered for. Land is at his best when he discusses at length the innovative programming of the Pacifica Network and describes the eccentric personnel responsible. One is left wondering how Hill, ever the paternal intellectual, would have viewed such developments. In 1967, Chris Koch made illegal programs for broadcast

from Hanoi, while Seymour Hersh first broke the Mai Lai story on the network in 1969. This book is therefore a useful addition to the literature on the media during the Vietnam War, especially Daniel Hallin's *The Uncensored War* (1989). Hallin suggests that the mainstream media were only able to challenge the prosecution of the Vietnam War once the political consensus had broken down after the Tet offensive of 1968. In contrast, both Land and Lasar remind us that serious challenges against the war by "underground" media peppered its duration. Lasar's discussion ends in 1964, and he is therefore in no position to consider in depth Pacifica's contribution to the development of the counterculture in the 1960s. Lasar, as we have seen, takes up the story and pursues it through the early 1970s, when new social movements emerged that turned Pacifica into a community radio network serving particular group interests. The station had been established as an intellectual challenge to commercialism. The audience it sought, however, was in fact alienated by its highbrow approach, and the station was transformed into an agency of social fragmentation. One glimpses in Land's book the genesis of the notion that Pacifica lost sight of the need for a radical voice once the Vietnam War ended.

This is where Land's book ends, and it is a shame; both books leave the reader with an incomplete picture. Neither volume is tempted to offer any conclusion, and in that sense, the story is incomplete. What has happened to Pacifica Radio since the 1970s? Have its policies and programming attitude been transformed yet again by its need to reflect and adapt to the ever-changing social-political climate? How has the Pacifica Network responded to the developments in the media environment that have given the community a louder voice within the public sphere? These are questions that future studies of the station will need to address. In the meantime, Land and Lasar provide sufficient background and context to enable readers to appreciate the evolving relationship between politics and the media in the second half of the twentieth century.

GARY D. RAWNSLEY
University of Nottingham

NANCY SHOEMAKER, *American Indian Population Recovery in the Twentieth Century*. Albuquerque: University of New Mexico Press. 1999. Pp. xiii, 156. \$39.95.

To many observers in the late nineteenth and early twentieth centuries, American Indians constituted a "vanishing race"—incapable of civilization, unwilling to accept assimilation, and thus doomed to inevitable extinction. In 1900, census takers counted only 237,000 American Indians, a demographic nadir wrought by centuries of intertribal warfare, climactic change, disease, and forced removal. While much scholarly research has concentrated on the various causes of what has been called an Indian "holocaust," Nancy Shoemaker's provocative study examines the causes and

consequences of the remarkable demographic recovery that occurred in the twentieth century, breaking new ground in an area that has largely been ignored by scholars of contemporary Indian history.

Applying quantitative methodology to U.S. Census data and Public Use Samples, Shoemaker concentrates on five tribes (the Senecas, Cherokees, Ojibways, Yakamas, and Navajos), peoples with diverse cultures, economies, and histories. It comes as no surprise, therefore, that Indian demographic recovery was not uniform—a theme that Shoemaker is careful to reiterate throughout the study.

In her "Overview of American Indian Demographic History," the author tackles two particularly sticky—yet connected—problems: what is an "Indian" and the reliability of census data. In 1900, for example, census takers used "enumerator observation" to discern the Indian population; in other words, they counted as "Indian" anyone whom they identified (through physical characteristics and place of residence) as "Indian." In later censuses, however, "self-identification" was used as the sole criterion (anyone who claimed to be an Indian was counted as one). Shoemaker addresses the inevitable "are we comparing apples to apples?" question by asserting that concern about changes in identity should not overwhelm researchers since there were natural increases as well. Although nagging questions remain about the reliability of the statistics employed throughout the book, the author is to be commended for pointing out the myriad of problems associated with compiling population histories (unreliable population data, migration, and shifting tribal identities).

The populations of the five tribes began a slow albeit uneven recovery during the first half of the twentieth century. The terrible influenza pandemic of 1918–1919 temporarily reversed the trend. Indian women married younger and began having children at an earlier age than whites and blacks, but they stopped earlier and spaced their children farther apart. Due in part to recurrent problems with tuberculosis, trachoma, venereal disease, diabetes, and alcoholism, Indian mortality rates in the early twentieth century hampered rapid demographic growth. Use of birth control and abortions also placed a check on Indian progeny rates.

The author challenges classical "Eurocentric" theories that posit economic factors as keys to explaining demographic patterns. Scholars have proposed that "modernization" (urbanization, democratization of education, increases in the standard of living, and medical advances) lowers mortality, and once parents realize that their children will survive to adulthood, they have fewer children. Regional development theorists on the other hand, point to land availability as having the greatest impact on family size. As land becomes scarcer, family size declines.

Shoemaker argues that these theories often fail to explain the diverse demographic experiences of Native peoples. Tribes with the highest school attendance, for example, had high fertility. That being the case, she

describes several economic and cultural conditions that account for the different demographic experiences of Indians. Intermarriage with whites, for instance, exerted an important influence on fertility. Mixed-marriage couples had higher fertility than full-blood couples, perhaps because they adopted Euro-American childbearing attitudes and had readier access to health care. Nevertheless, Navajos had little intermixture with whites but enjoyed high fertility because of their relative isolation from disease.

Attitudes toward the accumulation of wealth also help clarify the diversity of Indian demographic experiences. As the twentieth century wore on, some Indian peoples moved toward the acceptance of wealth accumulation as an acceptable cultural condition. Such acceptance led to a higher standard of living and higher fertility rates.

Shoemaker also points to household structure as an important influence on fertility. Those tribes that abandoned traditional extended families for nuclear families experienced higher fertility rates than those who continued to host grandparents, aunts, and uncles under one roof. Fewer mouths to feed—not to mention increased privacy—allowed couples more opportunity to procreate. Whether this change adversely affected life expectancy is unknown.

Indian population recovery accelerated after World War II. Improved health care, urbanization, and greater integration into the larger society led to increased instances of intermarriage with whites, and Indian families contributed their share to the postwar “baby boom.” Yet integration did not necessarily translate into social equality, as Indians arriving in cities from reservations often found themselves living in the poorest minority neighborhoods (p. 79). Indian wage earners could expect to earn less than half that of their white counterparts, and by 1979, approximately twenty-eight percent of Native families lived below the poverty line. Although the issue of increased population has been “less politically empowering than emotionally empowering” for Indians, Shoemaker demonstrates in a convincing manner that with a current population of two million, American Indians are “no longer at risk of being remembered in history” as the “Vanished Race” (p. 103).

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DAVID M. REIMERS. *Unwelcome Strangers; American Identity and the Turn Against Immigration*. New York: Columbia University Press. 1998. Pp. xii, 199. \$27.50.

Balanced treatments can reveal as well as hide. This is both a strength and weakness of David M. Reimers's synoptic account of the current debates around U.S. immigration. Reimers's claim to allow his subjects to “speak for themselves” (pp. 1, 2) brings clarity to the positions of both supporters and opponents of immigration, but it also conceals the author's voice that just as surely informs the writing of this text.

Reimers argues that U.S. immigration policy, both past and present, has been made in “an ad hoc and uninformed way” (p. 3), and a failure to deal with immigration and its attendant problems could lead to a backlash against immigrants. This book steps into the breach of a contentious and heated divide over immigration and American identity itself. Reimers traces U.S. immigration history from its unregulated beginning to its present, regulated though generous version. Immigrants were not counted until 1820, and states, he observes, regulated immigration until 1875, when the U.S. Supreme Court ruled that immigration was a matter of federal concern. Prostitutes and Chinese women, the latter commonly equated with the former by immigration officials, were denied entry in 1875, and the Chinese were the first racialized group excluded by Congress in 1882. Exclusions were extended to other Asians, and xenophobia and racism restricted the entry of Catholics, Southern and Eastern Europeans, and Mexicans up to World War II.

Congress relaxed restrictions during and after the war, and the landmark Immigration Act of 1965 greatly increased the numbers of immigrants and changed the nature of immigration from one dominated by Europeans to the present configuration wherein Asians and Latinos constitute most of the “new” immigrants. Public attitudes toward immigrants, Reimers shows, were complex, varied by race (although he leaves out gender and class), and changed with the emerging patterns of immigration. Arising from that mix were the “new” restrictionists, who contended that the U.S. had a broken immigration system and that immigrants depleted scarce resources, displaced native workers (especially the poor and African Americans), drew benefits from social programs, and harmed the environment. Those arguments, Reimers points out, resonate with historical precedents set during times of economic dislocation and social fragmentation, and he concludes that with so much passion on both sides, ad hoc policies will probably continue to characterize the nation's immigration laws.

This is a useful, succinct guide to negotiating the contemporary currents on immigration, along with their allied issues and concerns of assimilation and multiculturalism (the latter not discussed by Reimers). But his reliance upon partisan writings, both for and against immigration, as the principal narrators of this story weakens his analysis. (Other sources include government reports, newspapers, and interviews with the leaders from both sides of the debate.) For example, Reimers allows the proponents of immigration to level the charge of racism against the restrictionists without examining the truth or falsity of that allegation and without substantiating his belief that the new restrictionists were not racists (p. 1). Readers could accept the claim or reject it on the basis of their own judgments and prejudices, but they receive little help from the author to arrive at a more informed reading of that charge and denial. Similarly, Reimers leaves unexamined the arguments around the economic, so-

cial, and environmental impacts of immigrants, citing the need for further studies without researching those questions himself or working through the existing scholarship on those subjects.

Reimers, nonetheless, is unequivocal in his support of the current diversity of immigrants and in his belief that immigrants generally enrich U.S. society and the economy. At the same time, albeit without much evidence, Reimers claims that immigrants are detrimental to certain sectors of U.S. society, increase the crime rate, and take advantage of entitlements such as affirmative action over more deserving others. Government, Reimers proposes, should provide aid to communities adversely affected by immigrants, and immigrants, while keeping their own cultures, should learn English. Those beliefs, proposals, and their underlying assumptions could have provided the basis for yet another book.

GARY Y. OKIHIRO
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WENDY F. KATKIN, NED LANDSMAN, and ANDREA TYREE, editors. *Beyond Pluralism: The Conception of Groups and Group Identities in America*. Urbana and Chicago: University of Illinois Press, 1998. Pp. 281. Cloth \$49.95, paper \$19.95.

This edited collection is the result of a conference held at State University of New York's Stony Brook campus in June 1992. Some of the essays have appeared elsewhere and are not new. The conference itself aimed at considering multiculturalism and pluralism in a historical context free of angry debates that have surrounded these terms in recent years. The essays touch on the key issues, but overall they lack a coherent focus. Included are a speech by Bill Bradley on race and the American city, which he delivered in the Senate in 1992; essays dealing with racism, Africans, and American blacks; a piece on J. Hector St. John de Crevecoeur's *Letters from an American Farmer* (1782); and essays dealing with trends and theory. None deal with Latinos in any depth, and editors Wendy F. Katkin, Ned Landsman, and Andrea Tyree do not offer a convincing argument for the inclusion of these particular choices. The reader is left to reading essays that touch upon various aspects of the issues surrounding multiculturalism and pluralism.

In addition, like so many volumes of this sort, the book contains essays of uneven merit. Space precludes a detailed discussion of each piece; instead the highlights and strengths and weaknesses of most will be discussed here. The first excellent essay by Stanley N. Katz discusses how the American constitutional system historically has dealt with the claims of groups as opposed to individuals. Katz emphasizes that for most of American history, constitutional issues have referred to individuals and not groups. The Bill of Rights is a case in point. It reflected James Madison's and the Founders' worry over procedures and the rights of individuals. After 1865, Katz sees a shift as embodied

in the Reconstruction amendments. For many years, however, the courts used the Fourteenth Amendment to protect corporations and not African Americans, as was intended. Beginning with a case in 1938 (but, more importantly, with the 1954 *Brown v. Board of Education* decision), substantive equality entered the Supreme Court's jurisdiction. Although he sees a retreat from this new tradition in recent years, Katz believes that the key question is, "can constitutional democracy in the late twentieth century operate on the basis of having faith in groups other than one's own?" (p. 22).

Mary Waters's essay on "Multiple Ethnic Identity Choices" reflects her scholarship on ethnic identity. This is a first-rate summary of recent trends about intermarriage among all American groups. She notes that the American people have been intermarrying at increased rates in the last three decades, and this includes African Americans, even though their rates are lower than those of other groups. Waters sees this trend as the result of the success of the nation's pluralism, which has seen Americans interacting with one another in many parts of American society. The result, however, is that intermarriage complicates ethnic or racial identity—whether in the census, affirmative action, or group identity. She wonders how the children of intermarriage will identify themselves in the future. In Waters's view, the evidence thus far does not provide a simple answer.

David Hollinger argues for a postethnic society, one in which the democratic-egalitarian ideology becomes the basis of the nation. This society, which is frankly utopian to Hollinger, is one in which ethnic differences will remain but will not be important to one's identity. Hollinger's vision is one that many share, but how to reach that goal is a major question. He notes that many people of European background have already mixed, but unless economic opportunities are extended to poor minorities, a postethnic society will remain an unfulfilled goal.

Werner Sollors's essay on multiculturalism gives the reader an excellent guide to the debates on multiculturalism, political correctness, and the canon. He puts writers such as Dinesh D'Souza and Roger Kimball in context and notes how emotional the debates—often lacking in critical scholarship—have become. Indeed, the individual anecdote is often stated by one commentator and used time and again by others as an example of their "truth." History is cited by all sides in the multicultural discussions. Sollors notes that disagreements over these terms are often driven by ideology as scholars, politicians, foundations, and ethnic leaders line up to debate the issues.

Landsman contributes a nicely crafted essay on Crevecoeur's very limited vision. Clearly Indians and black slaves were not part of this new man being formed in the American colonies, and the peoples of Europe who were "melted" shared a Protestant view of society. As Landsman notes, by picking Pennsylvania rather than Virginia or South Carolina, he found an

apparently tolerant place. This is the only essay that goes back to the colonial era, and it seems out of place.

John Kuo Wei Tchen concentrates on New York City's response to Chinese immigration before the Civil War and in the last thirty years. His discussion of cartoonists such as Thomas Nast is enlightening, but his coverage of the post-1945 era is less compelling. Tchen sees many of the old stereotypes being used once again. His critique of Gwen Kinhead's view of Chinatown is on target, and his discussion of the "model minority" is good. He believes that the main difference between the nineteenth century and now is the elimination of racist laws that barred Asians from immigrating to the United States. But behind those changes are forces and conditions quite different than those that prevailed in the 1880s. The postwar decades cannot be treated in a few pages.

One of the outstanding essays is by Matthew Frye Jacobson, focusing on Jewish, Polish and Irish immigrants' responses to turn-of-the-century American imperialism, which, especially as it relates to the Philippines, was characterized by racism. He finds that, based on their experiences in the old world, these groups looked on the idea of revolution favorably. They wanted an independent Poland and Ireland and an end to anti-Semitic Tsarist policies in Russia. As a result, they identified with Filipinos and Cubans trying to control their own destinies. Such views put them at odds with the American thrust for empire. At the same time, they shared the racism so common then, and they wished to become "white." They believed that Filipinos were inferior, but they rejected American racism that claimed that the Poles or the Irish were a separate race and inferior to Anglo Saxons. Thus, some opposed American imperialism and others sought to become "Caucasian" in order to become respectable Americans. Jacobson's sources centered on leaders and the ethnic press, but one can probably safely assume that readers and followers held similar views.

Lawrence D. Bob and Ryan A. Smith's essay, "Jim Crow Racism to Laissez-Faire Racism," deals with changing attitudes by whites toward African Americans from 1940 to 1990. The authors see ample evidence for the emergence of racial toleration on the part of white Americans, but they note that polls also reveal a considerable amount of white racial antipathy toward blacks. The day of Jim Crowism with its accompanying racial ideology is past, they argue, but one can hardly overlook the segregated housing patterns of American cities and suburbs and the racial disparities in wealth, income, and health. We have entered an era in which the old racism is gone, but they characterize the new order as laissez-faire racism. This new style opposes programs such as affirmative action and other policies that might aid blacks. The view of Bob and Smith is hardly controversial.

Nikhil Pal Singh's essay, "Toward An Effective Antiracism," is long on theory and jargon, which gets in the way of a clear analysis of how to end racism. What are we to do with sentences such as "The effort

to reinvent an effective antiracism today out of the ample insights of the past, in other words, must draw from the resources inherent within those universalities that present themselves as alternatives to the unilateral declarations of the universality of capitalism that for its victims at least continues to radiate disaster triumphant" (p. 234).

The last two essays, by Kwame Anthony Appiah and Bradley, suggest remedies. Appiah is opposed to Afrocentrism and multiculturalism if the latter means simply teaching children about their own culture. Why, he asks, should we teach a particular group about its culture and not others about that culture? He prefers that all children learn something about one another (but not too superficially) and about the general history of the United States. It is hard to argue against this view, but it is easier said than done. Bradley is more specific with a program for aiding inner cities to have less crime and better housing, schools, and jobs. That, too, is easier said than done.

DAVID M. REIMERS,
EMERITUS
New York University

ROBERT C. LIEBERMAN, *Shifting the Color Line: Race and the American Welfare State*. Cambridge: Harvard University Press. 1998. Pp. xiii, 306. \$45.00.

MICHAEL K. BROWN, *Race, Money, and the American Welfare State*. Ithaca: Cornell University Press. 1999. Pp. xxii, 381. Cloth \$55.00, paper \$22.50.

Political scientists Robert C. Lieberman and Michael K. Brown agree that race penetrates the deepest roots of America's welfare state. While Lieberman finds in the history of Social Security the path through which the welfare state can leave behind its racially biased origins, Brown's emphasis on the limits imposed by the enduring influence of race and money gives no grounds for optimism.

Lieberman focuses tightly on the history of three programs—Old Age Insurance (Social Security), Aid to Dependent Children, and Unemployment Insurance—to show the impact of institutional structures and "policy feedbacks" on African Americans. Brown ranges more widely, brilliantly integrating the welfare state into the history of American federal politics since the 1930s. Lieberman combines published primary sources with ingenious quantitative analyses of program and census data; Brown bases his interpretation on research into unpublished sources in presidential archives and reanalyses of published quantitative data.

The three federal or federal-state programs on which Lieberman concentrates originated with the Economic Security Act of 1935. (Lieberman omits Old Age Assistance, until the mid-1950s the largest federal-state relief program created by the legislation.) Although each followed a different racial trajectory, Lieberman's story drives home a crucial point: even nominally color-blind policies have racial consequences.

In the 1930s, the South's desire for cheap labor imprinted itself onto the structure of America's welfare state through the statutory exclusion from Old Age Insurance of agricultural and domestic work, the major occupations of African Americans. Over time, administrative and statutory change broke down exclusions until, today, Social Security covers virtually all African Americans. The key to Social Security's expansion lay in its uniform national rules and administrative centralization within the national government. Aid to Dependent Children, by contrast, was a weak and decentralized program with a "parochial institutional structure" that allowed local authorities to discriminate against African Americans, first by denying them benefits and later by "making them the central focus of bitter political disputes over benefits, leading to crack-downs, retrenchment, and ever more race-laden welfare policies" (p. 9). As expected, African Americans fared poorly at the hands of local officials in the South; what is more surprising is their inequitable treatment in the North until politicians turned to welfare benefits as a form of patronage designed to win voter loyalty. Unemployment Insurance, a federal-state program, has fallen somewhere in the middle. It began with the same statutory exclusions as Old Age Insurance but shed them less successfully, largely because benefits, set by state governments, require the kind of steady work less often available to African Americans and permit disqualification for reasons open to subjective judgment.

Brown insists more than Lieberman that continued inequities mark *all* tracks in America's welfare state. In the New Deal, argues Brown, blacks gained access to the welfare state but only as relief recipients, "not as workers entitled to social rights." As a result, by the New Deal's end, dependence had gained its equation with race and guaranteed blacks' concentration in the stigmatized public assistance track of the welfare state. "This connection, rather than occupational exclusions or discrimination by local administrators," argues Brown in contrast to Lieberman, "was the crucial legacy of the Roosevelt policy settlement" (pp. 61–62). The distribution of current-day welfare state benefits still reveals the influence of this settlement. In 1986, Brown points out, black households received only 8.2 percent of all non-means-tested transfers (which include Social Security)—the welfare state's most generous benefit—but 32.3 percent of the much smaller means-tested payments (Aid to Families with Dependent Children [AFDC], Food Stamps, and related programs) that reflect the mean heritage of relief.

Brown criticizes the "polity-centered" school of welfare state historiography whose most visible proponent has been Theda Skocpol. Lieberman's interpretation also places him within this school. Its near-exclusive focus on political institutions, argues Brown, "begs the question of state autonomy and evades fiscal capacity" (p. 23). In fact, most historians, Brown contends, ignore the politics of fiscal constraints, which he puts at the core of his story. Even Franklin Delano

Roosevelt and Lyndon Baines Johnson, among American presidents the major advocates of an expansive welfare state, found themselves trapped by resistance to higher taxes and the need to retain business confidence. Since the 1930s, in both the North and South, the interaction of fiscal constraints with white desire to retain racial privilege has crippled the development of America's welfare state. Brown labels the incomplete structure that resulted—a combination of "comprehensive, universalistic policies for the elderly with an assortment of means-tested transfers, social services, and private benefits for the nonelderly"—"truncated universalism" (p. 4–5).

Brown also shows the imbrication of race in the immense world of employee benefits that developed after World War II and the urban housing and training programs central to the Great Society. Striking and controversial insights pepper his discussion of these other corners of the welfare state. Because of their exclusion from unions, contends Brown, most blacks remained outside the embrace of the generous benefits negotiated as a substitute for a universal, public welfare state. The result not only retarded the development of public benefits, as others have noted. It also drove a racial wedge into the working class and eroded the chance for a biracial working-class coalition (p. 164).

As for the Great Society, its job training programs, Brown argues, became substitutes for relief. They focused more on funneling income to out-of-work individuals than on equipping them with skills or providing them with jobs. In this way, they replicated the relief programs of the 1930s by holding individuals out of the labor force, thereby "rigidifying the racial bifurcation of urban labor markets" (p. 288). Although Brown departs from the focus on gender popular in recent welfare state historiography, he remains alert to its impact, and his observations about gender are trenchant. For instance, white and black women, he points out, have fared differently in the welfare state because white women "usually have multiple sources of income regardless of marital status" (p. 335). Nonetheless, the history of social policy since the 1930s, he believes, "revises" the idea emphasized by Linda Gordon that America's "dual welfare state is based solely on a distinction between a social insurance channel for white, male workers and a public assistance channel constructed for women" (p. 365). The reason is that after the 1930s, AFDC "was understood as a program for African Americans" (p. 366).

Despite their differences, Lieberman and Brown find a common moral in their histories. Race-neutral policies do not exist. In America, nominally color-blind policies, emphasizes Brown, in an argument very similar to Lieberman's, "have a way of being particularized along racial lines" (p. 372). Lieberman sees the possible transcendence of race only in national policies that override local parochialism. With no chance for the nationalization of public assistance in the current political scene, Lieberman is hopeful about the poten-

tial of some less ambitious but still useful programs. Brown lacks Lieberman's faith in institutional rules, which he finds continually undermined by the combined force of fiscal constraints and racism. Only the recognition by white workers of their common interest with blacks can reconcile the divisions of race and class and move policy forward in a progressive direction. Brown offers no reason to expect this transcendence of history anytime soon.

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J. MORGAN KOUSSER. *Colorblind Injustice: Minority Voting Rights and the Undoing of the Second Reconstruction*. Chapel Hill: University of North Carolina Press. 1999. Pp. x, 590. Cloth \$65.00, paper \$29.95.

J. Morgan Kousser concludes his study of voting rights legislation and litigation by noting the historian's duty "to set the story straight" (p. 467). The Supreme Court has built historical concerns into its voting rights jurisprudence, and Kousser insists that applying that jurisprudence well demands that courts rely on good historical practice as determined by historians' standards.

According to the Supreme Court, laws whose purpose is to exclude voters because of their race are unconstitutional, and laws defining electoral districts are unconstitutional if race was the predominant factor in drawing district lines. Kousser devotes one chapter to identifying ten factors courts should use to determine purpose and motivation. Notably, these factors are what good historians would use in writing narratives attempting to account for a law's enactment.

Kousser provides five case studies of voting laws that illustrate how a historian approaches questions of purpose and motivation. An extensive examination of the evolution of Los Angeles County districting demonstrates how lines were drawn to protect incumbents vulnerable to challenges from minority voters. Chapters on race-conscious districting in North Carolina and Texas argue that race was not the "predominant factor" in either state, despite the Supreme Court's conclusions in *Shaw v. Hunt* (1996) and *Bush v. Vera* (1996). Rather, a complex set of factors determined the district lines: "personal and partisan factors" (p. 296) interacted, "often producing unlikely coalitions because of the 'ripple effects' of changes in one district on the shape of another" (p. 247). Kousser is especially acerbic in criticizing Supreme Court justices, particularly Sandra Day O'Connor, for asserting that awkward-looking districts depart from "traditional" districting criteria such as compactness, demonstrating that such "traditional" criteria have typically been sacrificed when some more pressing political concern comes to decision makers' attention.

Two chapters examine departures from the American political tradition that the candidate who receives the most votes wins, even if he or she does not receive a majority (think of Bill Clinton, who received less

than a majority of the votes cast in 1992 and 1996). Chapters on the use of run-off requirements in Memphis, Tennessee, and Georgia show that racial considerations were "inextricably intertwined" (p. 191) with the political self-interest of the politicians who enacted the requirements. In Georgia, the requirement that a candidate receive a majority of the votes was adopted in part to keep African Americans and liberal whites from getting the Democratic nomination. Kousser's discussion here is particularly strong in using sketches of influential individuals and their views to establish a racial motivation with respect to matters about which there is no contemporaneous record.

The five cases studies at the book's heart sometimes betray their origins as reports Kousser prepared as an expert for litigation. Students of Los Angeles history may find the detail Kousser provides useful, but it may overwhelm those who come to the book with a more general interest in voting rights history. Kousser sometimes adopts a lawyer's rhetoric in his comments on academics and judges who disagree with his conclusions. Only occasionally does he acknowledge that some of the judges to whom his reports were submitted rejected his conclusions, although when he does he provides insightful comments on the reasons why were rejected.

Kousser devotes one long chapter to an explication and critique of the Supreme Court's voting rights jurisprudence, concluding that the redistricting cases are "revolutionary, contradictory, and incoherent" (p. 368). Again, much of this reads like a lawyer's brief, and indeed Kousser concludes with a doctrinal recommendation. Unlike a lawyer, however, Kousser is willing to draw the troubling conclusion that Justice O'Connor's often dispositive votes in the districting cases can best be explained as Republican partisanship.

The book's most valuable chapter compares the first and second Reconstructions. Kousser discerns a pattern in which minorities are protected when, and as long as, the political system is stable and changes occur gradually, which allows "minorities and their white allies to learn from and correct the inadequacies of previous policies" (p. 16). In this light, Kousser is especially disturbed by the Supreme Court's modern racial districting decisions, which disrupt the political stability that politicians had achieved.

Kousser presents his case, and his case studies, persuasively. Like any good historian, he is attentive to nuance and complexity, and when he concludes that the evidence lies conclusively on one side, his judgment carries real weight. This may leave us wondering about Kousser's insistence that "for the equal protection clause, history, and only history, matters" (p. 456). On his evidence, history as done by historians does not seem to matter that much to the courts. Perhaps when courts refer to history they are talking about something different from what historians refer to when they talk

about history. As a historian's work, this book is quite powerful; its implications for law are less clear.

MARK TUSHNET

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GEORGE LIPSITZ. *The Possessive Investment in Whiteness: How White People Profit from Identity Politics*. Philadelphia: Temple University Press, 1998. Pp. xx, 274. Cloth \$59.95, paper \$19.95.

Hank Aaron once made the elegant observation that, although most white Americans will concede that some black ballplayers in the age of segregation were "good enough" to have cracked the major leagues, few ever reflect upon the benefits that the color bar bestowed upon those white players who never had to face the Jackie Robinsons, the Bob Gibsons, or the Willie Mayses of their generation. How much did Babe Ruth owe to segregation's artificial narrowing of the competitive field? And what of those white players whose talents were merely middling or marginal? George Lipsitz's book represents Aaron's insight writ large. Traversing a remarkably broad terrain of American social, political, and cultural history from the colonial period to present, Lipsitz interrogates whiteness as an idiom of privilege and gain—a shared "investment" whose dividends for generations have accrued to white liberals and white reactionaries alike. Discrimination, we too easily overlook, means profit just as surely as it means loss: "minority disadvantages craft advantages for others"; "whiteness has cash value" (pp. vii, 12).

Over the latter half of the twentieth century, critical work on racism in the United States has steadily shifted from the individualized, bigotry-and-prejudice model favored by writers like Gunnar Myrdal in the 1940s, through a more systemic analysis of "institutionalized racism" in the 1960s, to a view of "race" itself as a socially constructed articulation of power in the 1990s: a mutable set of conceptions and perceptions at once shaped by, and expressive of, patterns of dominance and resistance within the culture at large. Lipsitz's book both consolidates and advances this last turn in the scholarship.

Lipsitz lays the basis for his analysis in a compelling pair of opening chapters on the structural features of white privilege in the U.S. context. Housing policy and real estate practices, banking and finance, education, tax codes and subsidies, the behavior of the courts, and the norms of urban policing are all heavily infected by a racist logic or tend toward racialized consequences. Most powerful in this discussion are Lipsitz's closely entwined contentions that *all* white people have reaped the benefits of whiteness; that many have been able to pass—and so to augment—these race-based profits from one generation to the next; and that, far from affording equal protections, the state itself actually stands at the very center of this story. The capital represented by a house in the suburbs, for instance, has proven the key to many white families' economic mobility, although few white Americans recognize that

segregation has historically been the guarantor of suburban real estate values and that federal policy, in its turn, has underwritten the prevailing patterns of *de facto* segregation. Proof of whites' failure to reflect on these matters, for Lipsitz, is their "principled" distaste for the notion of "government handouts" distributed according to race.

Building on the powerful logic and commitment of this opening discussion, in successive chapters Lipsitz takes a variety of angles on the workings of whiteness. He offers provocative readings of the racial valences of U.S. foreign policy; the power of "normative" whiteness in both the media coverage and the courts during the O. J. Simpson trial; "authenticity" and racial romance in bluesman Robert Johnson's popularity among white audiences; the racialized, reactionary appeal of Paterson's baseball bat-wielding high school principal, Joe Clark; the political flights of Dizzy Gillespie's fanciful "Swing Low, Sweet Cadillac"; and the wages of whiteness in Pete Wilson's California, "The Mississippi of the 1990s." All of these discussions are productive: some of them are dazzling.

Some readers might be put off by this book's peculiar narrative line. One has to retrace one's steps occasionally in order to figure out just how it was that Lipsitz got, for example, from an analysis of the film *Lean on Me* (1989) to a eulogy for historian George Rawick. It is not altogether clear whether its meandering line reflects the book's narrative strategy or merely the circumstances of its production as a cobbling together of existing essays. But this roving quality also generates a subtle power. These narrative turns create the dual impression that, first, there is virtually no corner of American politics, society, or culture where the discerning eye will fail to discover evidence of "race" and its workings; and, second, anywhere Lipsitz casts his gaze he will find something interesting and insightful to say. I have cause to question neither conclusion. This is a terrifically important book.

MATTHEW FRYE JACOBSON
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ALAN DERICKSON. *Black Lung: Anatomy of a Public Health Disaster*. Ithaca: Cornell University Press, 1998. Pp. xiv, 237. \$22.95.

In the nineteenth century, physicians diagnosed coal miners who spit up black ink and suffered lack of breath with the occupational disease of "miner's asthma." Beginning in the first decades of the twentieth century, miner's asthma petered out of existence. Medical experts now attributed black sputum and shortness of breath to tuberculosis, smoking, and poor personal hygiene. To the extent that physicians and public authorities did recognize an occupational disease among miners, the disease was "silicosis," but since few coal mines were dug through rock with appreciable quantities of silicates, few coal miners were thought to suffer from silicosis. Then, beginning in the 1960s, a new occupational disease of miners,

"black lung," was understood to produce their inky sputum and disordered respiration.

In oversimplified outline, this is how Alan Derickson's book illustrates the principle of the social construction of disease. Long-term coal miners have always spit ink and struggled to breathe, but the "disease," if any, that caused their suffering changed over time. More important, Derickson documents a conscious campaign by twentieth-century coal mine operators to "disappear" miner's asthma. Their motive was financial. Operator negligence killed miners in their midlives, but to prevent the expenses of prevention and compensation, operators refuted the existence of an industrial disease among coal miners. With the assistance of many medical scientists, the operators successfully repressed social knowledge of miners' maladies. Workers won little health and safety regulation and few tort cases or compensation hearings, blocked by their inability—within the confines of accepted medical theory—to prove they suffered harm from working in dusty coal mines. The book is a searing indictment, then, of both mining and medicine.

Perhaps the least satisfactory part of the book has to do with the physicians and other scientists who collaborated with mine operators against workers. Certainly, those who worked directly for the mines had financial incentive to accede to their employers' wishes. Derickson's barely argued suggestion that class collaboration along with monetary incentive explain the dearth of medical experts, among both mine employees and independent medical scientists, willing to challenge the scientific consensus that coal dust posed no danger to miners' health seems insufficient. Perhaps lack of data has forced Derickson to condemn the allied-health professions with so little explication, but even speculation might have satisfied our need for greater understanding.

The last third of the book traces the emergence of the new disease, "black lung." As in the disappearance of "miner's asthma," a deliberate campaign explains the change. This time, a few sympathetic physicians, some brazen Vista volunteers, and a resistance movement challenging complacent union leadership exploded into a grassroots movement. Militant miners used wild-cat work stoppages and confrontational protest demonstrations successfully to challenge the governmental and medical paradigm in which the only recognized dust disease was silicosis. Derickson explicitly evokes the Kuhnian model of "normal science" finally shattering under the accumulated weight of contradictory evidence, but he interprets the categories of accepted wisdom and heretical quackery within a political framework of class interests and populist protest.

This is a complicated tale with so many characters that I wished for a *dramatis personae*, as in a Gothic novel, to keep them all straight. Indeed, the book has certain Gothic qualities: secrets and plots, villains and heroes, dark, dusty workplaces and smoky political backrooms. Even the cover is literally black. Like the

physician-reformers who ghoulishly crumbled sections of deceased miners' coal-impregnated lungs before flinching audiences, Derickson's dissection of this public health disaster leaves the reader cringing.

Yet the book is not written in a romantic or popular style. It is a solid professional history. Derickson's story is well documented with an impressive range of published sources, archival documents, and oral interviews. His tone is dry, barring a few appropriately sarcastic comments, and his arguments, if impassioned, are impeccable.

This book is an impressive contribution to occupational health history, to labor history, to medical and public health history, and to United States history in general. Derickson's theoretical underpinnings—his employment of the Kuhnian "normal science" model, appropriately bolstered with an appreciation of embedded interests and structural conflict—work to explain shifts not only in scientific theory but in American political consensus and dissent. Indeed, this case study illustrates the intimate entwinement and mutual bolstering of scientific and political orthodoxy, but it reveals also that chinks in the armor of privilege can be pried open by populist challenges.

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DANIEL HOROWITZ. *Betty Friedan and the Making of The Feminine Mystique: The American Left, the Cold War, and Modern Feminism*. (Culture, Politics, and the Cold War.) Amherst: University of Massachusetts Press. 1998. Pp. viii, 354. \$29.95.

As most of us who teach U.S. women's history know, there is relatively little scholarly literature on feminist activism of the 1960s and 1970s, especially when compared to other twentieth-century political movements of comparable significance. For that reason, I was anxious to read Daniel Horowitz's book. An intellectual powerhouse with an ego sized to match, Betty Friedan is one of the most intriguing and frustrating of the post-World War II feminist leaders. Her career highlights both the astonishing momentum that drove the American women's movement from the early 1960s to the mid-1970s and the raging battles over ideology and leadership style that so often threatened to derail it. Horowitz makes a convincing argument that Friedan's life also illuminates previously unexamined links between the ideology of the Old Left and liberal feminism in the 1960s.

Rejecting Friedan's self-presentation as a trapped housewife who came to her gender analysis only as a result of her deep frustration with married suburban life, Horowitz roots Friedan's feminism in the ferment of the late 1940s, when Friedan was working as a labor journalist. Popular Front feminists, who published in many of the same journals that Friedan wrote for and who traveled in the same political and social circles, were then publishing essays and books that placed women at the center of contemporary and historic

movements for social change. These Socialist and Communist-affiliated women also provided a theoretical framework about gender on which Friedan would later build. Betty Millard, Elizabeth Hawes, and Mary Inman, among others, expanded on Friedrich Engels's theories of the relationship between private property and the oppression of women, examined the gender socialization of children, and attributed economic value to the unpaid labor of wives and mothers.

The ideological lines that Horowitz draws from Popular Front feminism to the women's movement of a generation later constitute one of the important historical contributions made by this nuanced and detailed book. His attempts to understand why Friedan later tried to erase her radical past are somewhat less satisfying, not because of any lack of subtlety or sophistication in Horowitz's analysis but because Friedan herself has always been such an ambiguous figure, and because Horowitz's book examines only her pre-1963 career as a writer. Friedan's stormy and important "second career" as founding president of the National Organization for Women (NOW) and as the most visible face of American feminism from 1963 through 1970 receives virtually no attention in this book. That is a shame, because I think that both Horowitz's analysis and our historical understanding of Friedan would be enhanced by extending his perceptive portrait of a brilliant, complex, and deeply conflicted woman haunted and driven throughout her life by profound ambivalence about her Jewish background, her middle-class origins, her yearnings for an ideal heterosexual union, and even her feminism.

Horowitz's narrative offers many insights into the nature and origins of Friedan's ambivalences, beginning with her Peoria, Illinois childhood. She was born Betty Goldstein, privileged daughter of an upper-middle-class Jewish family. Midwestern anti-Semitism, Horowitz argues, sparked her first interest in social justice issues. Faculty at Smith College later schooled her in Freudian and Marxist analysis and provided exposure to the hotly contested labor struggles of the late 1930s. Her consciousness of social inequities was sharpened by her dismay at the genteel anti-Semitism of some of her classmates and even more by their disdainful treatment of the working-class women who served them food and cleaned their rooms. She voiced her concern over these and other issues in writing, as she would throughout her career, becoming the editor of a progressive campus paper.

Friedan continued her journalistic career after a short stint as a graduate student in psychology at Berkeley, working first for a labor-affiliated news service and then for the *UE News*, a publication of the progressive United Electrical Workers' Union. These jobs brought Friedan into contact with an impressive array of progressive writers and activists, among them the women through whose writings she was introduced to the language of gender analysis. Her dismissal from the news service after the war, her combative marriage to theater producer Carl Friedan, and the loss of her

next job at *UE News* when she became pregnant confirmed a growing disillusionment with male-dominated radical movements and deepened her interest in women's issues. That shift was also caused by her fear of public association with Old Left comrades in an era of mutual accusation and recrimination.

Thus began the suburban mom period of Friedan's life, during which she sought to eke out a living as a freelance journalist while raising three children, first in an international and racially integrated Queens neighborhood and then in the bohemian and intellectual suburb of Grand-View-on-Hudson. Horowitz details Friedan's role in suburban housewives' rent strikes, tenant demonstrations, and education-related organizing during the years leading up to the publication of *The Feminine Mystique* (1963). These accounts, and articles that Friedan herself published in women's magazines during the 1950s, lead Horowitz to affirm those scholars who have suggested that Friedan knowingly painted an overly passive picture of the lives of middle-class housewives in the postwar era.

The reasons for Friedan's dissembling are hard to unravel. So, too, are the sources of Friedan's abiding homophobia. Horowitz acknowledges but does not ultimately grapple with that issue, although fears of a homosexual menace twisted Friedan's political analysis from her days at Smith into the 1970s, when she almost tore apart NOW by insisting that lesbians in collusion with the Federal Bureau of Investigation and Central Intelligence Agency were plotting to discredit the women's movement. Horowitz rightly notes the influence of neo-Freudian psychology on Friedan's sense of self and on her writing. Undergoing therapy in the 1950s, she became convinced that her years as a radical represented an "inauthentic" self, and so she felt it a healthy step to distance herself from that past. Perhaps Friedan, like so many others, suffered lasting damage during the witch-hunting 1950s. It is equally possible, Horowitz notes, that Friedan's self-mythologizing was the conscious choice of a freelance journalist trying to sell her work to a broader public. Or maybe it was the strategic decision of a brilliant polemicist. Despite, or perhaps because of its tight focus on white middle-class women and their discontents, *The Feminine Mystique* remains in print and sells briskly decades after its initial publication. Unanswered questions about its author bubble up from my students every time I teach it. Horowitz's rich narrative sheds light on many aspects of Friedan's compelling and troubling career. Still, I hope that Horowitz will turn at some point to the second half of Friedan's life, when the contradictions in her politics and her personality played out so dramatically on the stage of the women's movement that she helped to create.

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LEILA J. RUPP, *A Desired Past: A Short History of Same-Sex Love in America*. Chicago: University of Chicago Press. 1999. Pp. xii, 232. \$22.00.

LILLIAN FADERMAN, *To Believe in Women: What Lesbians Have Done for America—A History*. Boston: Houghton Mifflin. 1999. Pp. xii, 434. \$30.00.

It is a sign of the healthy growth of gay historical studies that these two books are aimed at a general audience. Both Leila J. Rupp and Lillian Faderman were among the first to write on the history of lesbians: both have distinguished records of uncovering forgotten documents that highlight the complex ways in which sexual desire can impact politics, literature, and, indeed, the course of history. Both Faderman and Rupp insist that our present either/or categories of sexuality were inconceivable in the past, when definitions and behavior were more fluid. Certainly religions condemned the male sodomite and the mannish woman, but the enforcement of laws against sin were often erratic before the late nineteenth century, when medical typologies gradually began to dominate the public discussion of sexual behavior. Well into the twentieth century, many voices called for greater sympathy and “softness” among men, as well as more forthright honesty and “male” reason among women. And, as both point out, middle-class men and women often did not see their romantic friendships in these early definitions of deviant sexuality.

But Faderman and Rupp disagree about when and how to use such a word as “lesbian.” The former opts for seeing it as an adjective to describe women’s “committed domestic, sexual and/or affectional experiences,” arguing that “lesbian arrangements freed . . . pioneering women to pursue education, professions, and civil and social rights for themselves and others far more effectively than they could have if they had lived in traditional heterosexual arrangements” (pp. 1–2). Rupp is less certain that we can assign present-day labels to the past. Using the example of her aunt’s lifetime friendship with another woman, she unravels the textual complexities historians face when writing the history of same-sex desire. Rupp resolves the issue by using the phrase “same-sex love and sexuality,” even as she admits that we often do not know what “sexual” itself means. But she does see “certain common patterns in same-sex sexual desires and acts, romantic liaisons, and gender transgressions across time and place” (p. 10).

Faderman’s book is organized around the four major feminist causes of the nineteenth and early twentieth centuries: namely, female suffrage, the care of the poor and needy, the establishment of higher education, and the struggle to open the professions to women. She examines each movement through the lens of same-sex attachments, documenting how leaders were supported by special friends. She has culled many collections to good effect, quoting letters that have often been expurgated in the past to show how these women openly mixed love and politics. Faderman has an acute eye for the complex ways in which same-sex love can release creative energy. For example, the African-American Shaker, Rebecca Cox Jack-

son, was empowered to play a leadership role through her mystical visions about her beloved friend, Rebecca Perot. After Jackson’s death, Perot assumed leadership and took on the name Rebecca Jackson, Jr.

Faderman sees same-sex love as the essential underpinning of the first wave of feminism. For her, discontent with the predominant definition of gender roles led to a profound dissatisfaction with the existing heterosexual norms, with marriage, and with male privilege. Obviously, she acknowledges, we cannot know which came first: the ambition to open new fields for women, or the decision not to marry but to dedicate one’s life to social change on behalf of women. Only single women—with a supportive woman friend—could devote the necessary long hours to a nascent, unpopular cause. Faderman provides a valuable reminder of the longstanding importance of female nurturance, which she also concedes was often sexual in nature among feminist activists.

Faderman’s paradigms are clear and her chronology somewhat simplified, as befits a book aimed at the general reader. More troubling, however, is her explanation for the decline of feminism in the 1930s; she blames the rise of homophobia, generated by the medical profession and confirmed by a generation of women who did not appreciate what their foremothers had done for them. To her mind, these two conditions destroyed both same-sex friendships and the women’s movement. But “homophobia” is a complicated matter, and definitions of sexual identity can vary enormously over a lifetime for both individuals and society. Faderman has no explanation beyond “exceptionalism” for the flourishing network of unmarried professional women who were supported by and supported Eleanor Roosevelt through the 1930s and 1940s. Nor is it entirely true that single women’s friendships were admired and not stigmatized before the 1910s and 1920s. The very schematization that makes this so clear a book for novices may lessen its impact among specialists.

In contrast to Faderman, Rupp’s book includes gay men as well as lesbians and starts from the beginning of American history. At the risk of sounding dated in ten years, she presents the most up-to-date perspective on the history of homosexuality. Unlike Faderman’s careful culling of archival sources, Rupp has limited herself to surveying and synthesizing current work in the field. Each chapter begins with a personal anecdote and then moves gracefully into summarizing various aspects of same-sex desire at a particular historical period. Rupp’s more flexible approach to her subject permits her to explore the many different reasons why defined sexual subcultures developed for both men and women in the nineteenth century. She also provides a rich, albeit brief, survey of such different twentieth-century communities as urban Harlemites who reveled in their effeminacy and mannish women in Salt Lake City between the wars who survived through a combination of discretion and respectability. “Homophobia” is not the focus of her study, although Rupp never

minimizes the dangers (and allure) of an "out" life during periods when so-called sexual deviancy was under public scrutiny. Her deft and careful unraveling of the varied worlds of same-sex behavior will provide students with an excellent introduction to the current state of the history of homosexuality in America.

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JERRY LEMBCKE. *The Spitting Image: Myth, Memory, and the Legacy of Vietnam*. Assisted by HARVEY J. KAYE. New York: New York University Press. 1998. Pp. xi, 217. \$24.95.

Among the cultural images that create historical meaning about the American experience of the Vietnam War, that of the Vietnam veteran who was spat upon by antiwar protesters carries particular weight. Recent analysis of the effects of the Vietnam War in American culture has revealed that the dominant cultural narrative of Vietnam veterans has to do with their treatment after their return from the war rather than their experiences in the war. So, for example, the symbolic recognition of the veterans at the Vietnam Veterans Memorial in Washington, D.C. (a memorial for the dead that is also specifically about the living) can be said to have offered recompense to veterans much more on the issue of their treatment at home than their service at war.

Jerry Lembcke's book looks at this image of the veteran who, having served his country and barely survived, is spat upon by representatives of the antiwar movement, and finds it to be an urban myth. Lembcke works hard to show that there is no evidence of such incidents taking place, and examines why this image nonetheless carries such tremendous cultural currency. He effectively demonstrates that, in fact, many Vietnam veterans turned against the war and formed important coalitions with the antiwar movement. Moreover, he pinpoints the ways in which the Nixon administration, and later the Bush administration during the Gulf War, had enormous stakes in depicting the antiwar movement as anti-veteran, effectively rewriting the cultural memory of protesters demonstrating at induction centers while recruits and draftees went off to war into a story of protesters demonstrating at airports when the worn veterans returned home. Lembcke makes clear that the government's stakes in this representation were high, in particular in terms of garnering public support for the Gulf War by convincing Americans that not supporting the troops was a fatal mistake of the Vietnam War. Once those hundreds of thousands of American troops were sent to the Persian Gulf, this narrative effectively helped to shut down public debate on the war's merits.

Using this image as his focal point, Lembcke, who is a veteran himself, examines the ways in which the spurned veteran has functioned as a myth that both aided in the stereotyping of the veteran as a mentally

unstable psychopath and in demonizing the antiwar movement and the political Left. He covers this issue from many angles, discussing the role that well-meaning psychiatrists, in designating post-traumatic stress disorder (PTSD) as a veteran ailment, played in perpetuating the myth of the veteran as isolated and unstable and analyzing the gendered aspects of the myth that women spit on men. Lembcke is at his most effective when examining the compelling history of the work of veterans against the war, the ways that the Nixon-Agnew administration created a public discourse of good and bad veterans, and the long-term effects of this myth on subsequent antiwar movements.

In taking on a topic that is both narrow (the specific image of the spat-upon veteran) and potentially vast (the cultural memory of the Vietnam veteran), Lembcke is faced with a particular set of challenges, and at times the book seems to be arguing not simply that no veterans were spit on by anyone but also that no veterans were ostracized after the war or mentally damaged by it. Furthermore, in arguing in detailed fashion about whether or not actual spitting took place, or, for instance, giving a detailed analysis of why the film *Coming Home* (1978) distorted the stories of the veterans on whom it was partially based, Lembcke seems at times to be arguing against himself. For as he makes clear, the cultural memory of the spat-upon veteran, which some veterans themselves remember, is, even if a kind of false memory, nonetheless still a memory that has cultural value: it acts as a symbol for the difficulties experienced by the veterans after the war and the invisibility they felt. At other times, Lembcke is skilled at walking that ever-important line between dispelling the myth and acknowledging the difficulties that do exist for many veterans. His book is an important contribution to creating a more complex analysis of the experience of Vietnam veterans and, importantly, of the ways in which the image of the Vietnam veteran (and the Vietnam MIA/POW) has been exploited to aid in furthering warfare.

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GORDON SILVERSTEIN. *Imbalance of Powers: Constitutional Interpretation and the Making of American Foreign Policy*. New York: Oxford University Press. 1997. Pp. xi, 276. Cloth \$49.95, paper \$18.95.

Gordon Silverstein examines the role of the three branches of the national government in the making of American foreign policy. His primary focus is the post-World War II era, and in particular the congressional challenge to presidential prerogatives in foreign policy that culminated in the 1970s. From the perspective of a diplomatic historian, Silverstein's attention to the constitutional implications of the subject is welcome indeed.

Silverstein devotes about eighty pages, roughly one-third of his text, to the period up to World War II. Coverage is selective and more cursory than most

historians will like. However, they may find themselves stimulated by his provocative argument that foreign affairs rarely occasioned disagreements between Congress and the presidency over constitutional questions of authority. John Marshall developed a fairly consistent doctrine, which Silverstein labels the court's "traditional interpretation," that foreign policy fell into the realm covered by the Supremacy Clause. Silverstein realizes that such a formulation left many issues unanswered, but by his account contemporary criticism centered on the fear that the national government might use its latitude in foreign affairs to justify enlarging its powers at home. Much later, to meet this threat, Justice George Sutherland formulated a highly influential argument in the *Curtis-Wright* case (1936). Sutherland agreed that the Constitution allowed the national government almost unlimited sovereignty over foreign affairs, but he insisted that the various checks and balances, most notably the powers reserved to the states, restricted national power in domestic matters. Thus was laid a doctrinal foundation for postwar conservatism.

In 1941, Franklin D. Roosevelt began to assert, more by action than by argument, an executive prerogative in foreign policy. Silverstein contends that many Americans came to believe that, in effect, the Constitution, and not simply politics, should stop at the water's edge. Cold War presidents would act abroad without so much as consulting Congress. Moreover, Harry S. Truman and his successors fulfilled Justice Sutherland's worst fears by using presidential authority over foreign affairs to justify domestic action. Silverstein says that legislators welcomed presidential initiative so long as the action was aimed overseas. This support was, however, more ambivalent than Silverstein implies, as the near passage of the Bricker Amendment demonstrates.

Presidential assertion of this "prerogative interpretation" culminated during the Johnson and Nixon administrations. Silverstein's discussion of such key subjects as the Tonkin Gulf Resolution and the secret bombing of Cambodia is sensitive and informed. That congressional efforts to reassert authority have largely been a failure is conventional wisdom; that they have been counterproductive from a constitutional standpoint is a far more original contribution.

Silverstein contends that legislation like the War Powers Act, by granting presidents a grace period of unilateral action, in effect ratified the very existence of executive prerogatives that Congress hoped to restrict. Since Congress has proven unwilling to confront chief executives, such legislation has in effect expanded presidential power. The Supreme Court will back Congress only when legislation contains explicit restrictions, which Capitol Hill has consistently declined to enact. Seeing no hope that Congress will assert itself, Silverstein follows Richard Neustadt in suggesting that presidents may themselves wish to limit their power in the interests of protecting the office from the vulnerability of unilateral action.

A number of defects mar this book. The argument is frequently repetitious. The use of historical sources is very limited and contributes to a weakness in argumentation. Silverstein emphasizes that a presidential prerogative in foreign policy has been cited in recent constitutional arguments to justify expanding executive authority in the domestic realm. But the key shift in the Supreme Court's attitude about executive power came during the New Deal, when the issue was economic policy. Silverstein also overstates the case for a consensus "traditional interpretation" of the Constitution on foreign policy issues. The fight over Article 10 of the Versailles Treaty, for example, was at heart constitutional. Silverstein himself notes instances, like the Hostage Act of 1868, which do not indicate consensus, but these admissions tend to come after the fact. Most centrally, it is at least arguable that wider circumstances have shaped opinion in Congress more than its concern for institutional prerogatives. If true, the end of the Cold War may affect the current imbalance of powers.

These faults aside, the book has real value for its provocative arguments and for the light it sheds on the issue of Congress's modern role in making foreign policy.

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ANNE HESSING CAHN, *Killing Detente: The Right Attacks the CIA*. University Park: Pennsylvania State University Press. 1998. Pp. viii, 232. Cloth \$35.00, paper \$17.95.

The theme of this book is that the right wrecked detente in the mid-1970s by attacking the Central Intelligence Agency (CIA), with the result that America embarked on inflated military expenditures to the detriment of social spending. This focus stems from the author's background: Anne Hessing Cahn was Chief of the Social Impact Staff at the U.S. Arms Control and Disarmament Agency (ASDA) from 1977 to 1981.

Cahn makes no secret of her resentment at the slashing of the ASDA's budget but does her best to present a balanced picture. She acknowledges that it was not just the right's attack on the CIA that caused problems for detente. Unspecified "Soviet actions" had an impact. Other factors were the effects on American opinion of the arrival of Russian dissident novelist Aleksandr Solzhenitsyn in 1975, the mid-1970s hostility of organized labor to any deal with the Soviets, and Ronald Reagan's hawkish 1976 challenge to incumbent President Gerald R. Ford for the Republican Party's presidential nomination.

"Russians." That was H-bomb pioneer Edward Teller's early 1960s reply to a query as to what the Americans would find were they to land on the moon. The disposition to exaggerate Soviet prowess and assets was not new in the mid-1970s, nor was skepti-

cism about the CIA: Cahn notes that the consensus behind the CIA had been evaporating long before the events she describes. Here, then, the right can again be exculpated from sole responsibility for the mid-1970s crisis in confidence. Nevertheless, matters came to a head then, and Cahn utilizes a mass of evidence, much of it derived from use of the Freedom of Information Act procedures, to describe the process.

Here her account again stops short of polemicism. Instead of defending the CIA blindly against its detractors, she shows herself to be aware of the difficulties of estimation and of the possibilities for error. For example, in calculating the growth of the Soviet economy, which base date should one use? Ought one calculate the strength of the Soviet armed forces by counting their hardware or by computing its ruble costs in spite of Russia's command economy? Particularly in respect to the latter, Cahn notes that the CIA did not always get things right and had to admit the error of its ways.

Nevertheless, Cahn shows that the right did mount an effective attack on the CIA, culminating in the appointment of Team B, a committee of experts charged with looking at the evidence available to the CIA's Soviet analysts. That Team B came to alarming or, depending on one's opinion, alarmist conclusions about Moscow's intentions and capabilities is not a startling revelation. As long ago as 1977, historian Lawrence Freedman told the story in *U.S. Intelligence and the Soviet Strategic Threat*. But the virtue is in Cahn's detail, and in her perspective. Right down to the 1964 Clos de Vougeot consumed on the occasion, Cahn relates the story of the dinner party on the evening of June 4, 1974, at which Roberta and Albert Wohlstetter planned the fall of the CIA. Paul Nitze was there, and other luminaries ranging from Jesse Helms to the top brass of the Defense Intelligence Agency later joined the crusade.

Cahn implies that the right's campaign and the pressure exerted by Team B affected the National Intelligence Estimates on the Soviet Union. She is uncharacteristically short on detail here, and she is deficient on the logic of social expenditure cuts stretching into the 1980s; after all, President Reagan would not have supported social programs, even if the money had been there. Yet she succeeds in showing that the right's attack on the CIA helped to persuade America to spend billions superfluously: the Soviet Union was in reality crumbling, and there was no need to renew the arms race. Cahn demolishes the "victory" thesis linking the end of European communism to increased U.S. arms expenditure. This is a provocative book that makes a significant contribution to the historical debate on a vital issue.

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MARY PATRICE ERDMANS, *Opposite Poles: Immigrants and Ethnics in Polish Chicago, 1976-1990*. University

Park: Pennsylvania State University Press. 1998. Pp. x, 267. Cloth \$50.00, paper \$19.95.

The city of Chicago, to the present day, maintains its status as the most powerful magnetic attraction for Polish immigrants of any major urban area in America. The 1990 census reported a total of 9.3 million persons of Polish ancestry in the United States, ten percent of whom resided in the state of Illinois. The Chicago metropolitan area incorporated nearly 900,000 individuals of Polish ancestry, making Chicago Polonia the largest Polish settlement in the world outside of Warsaw, the capital of Poland. For this reason, numerous scholars in the twentieth century have attempted to describe the sometimes painful processes of assimilation experienced by Poles in Chicago and elsewhere. William I. Thomas and Florian Znaniecki's multi-volume classic, *The Polish Peasant in Europe and America* (1919), was largely based on data which focused on the infrastructure of Polish fraternal organizations: namely, the Polish Roman Catholic Union of America (PRCU) and the Polish National Alliance (PNA). The intense competition between the Unionists and Alliancists was duely noted by Thomas and Znaniecki, as well as by Wacław Kruszk, the priest-historian of the landmark *Historia Polska w Ameryce* (1905-1908), issued in a new four-volume translation by the Catholic University of America Press under the title *Polish History in America to 1908* (1993-1999). During World War II and the immediate postwar era, the work of Mieczysław Haiman provided a bridge between the early classic accounts of Kruszk and Thomas and Znaniecki and the "ethnic persistence school" pioneered by Victor Greene in *For God and Country: The Rise of Polish and Lithuanian Consciousness in America* (1975). Greene's work was accompanied by Edward Kantowicz, *Polish American Politics in Chicago, 1888-1940* (1975), this reviewer's *Polish Catholics in Chicago, 1850-1920: A Religious History* (1981), and Dominic Pacyga, *Polish Immigrants and Industrial Change: Workers on the South Side, 1880-1922* (1991). However, none of these histories covered the post-World War II experience of Cold War Chicago Polonia.

The monograph produced by Mary Patrice Erdmans, a sociologist, therefore breaks new ground. She focuses on the intragroup conflict within Chicago Polonia in the period coinciding with the rise of the Solidarity (*Solidarność*) movement in Poland and, in particular, on three major cohorts: Polish Americans (those born in America whose ancestors were born in Poland); the World War II emigrés; and the "new Polish immigrants" of the Solidarity migration and generation. In her introductory chapter, Erdmans argues that the Polish-American community in Chicago forged a coalition with the World War II migrant group "around their mutual distrust and hatred of communism (p. 10)." Moreover, the wartime experiences of both these groups reinforced established community-wide conceptions of the meaning of "Pol-

ishness" in American society, while at the same time adding newer ingredients to the collective sense of Polish "ethnic" identity that incorporated shared wartime ordeals. But, as has been the case for most ethnic groups in American history, older settlers—the "ethnics"—often have disagreed with more recent immigrants over goals, priorities, strategies, and tactics. In the Polish case, the intragroup divisions became particularly intense in Chicago. Who, then, were these Poles in opposition to the Chicago Polonia establishment (hence the book's title)?

They were primarily labor activists in the Solidarity movement of the early 1980s who initiated an exodus of numerous Polish citizens then suppressed by the Polish (and still Communist) government. Many of these activists migrated to Chicago Polonia, where they unveiled a radical political agenda. When this agenda was not immediately accepted by the established Polish-American fraternal organizations, especially the Polish-American Congress (which urged patience and caution), Erdmans contends that a rather bitter conflict occurred between the established Polish American "ethnics" and the recently arrived generation of exiled "immigrants." The Solidarity emigrés felt that the established Polish American organizations and leadership were overly concerned with domestic issues; some Solidarity members were appalled at the lack of interest in Chicago Polonia in current Polish affairs and events. In effect, the Solidarity exodus viewed itself as truly representative of legitimate Polish concerns. Some even attempted to wrest away leadership positions in Polish-American fraternals, much to the chagrin of the "ethnic" incumbents. The book concludes with a chapter on the critical 1989 elections in Poland, which eventually led to the downfall of Communist rule.

In support of her case, Erdmans relies heavily on extensive fieldwork in Chicago Polonia conducted between 1986 and 1992. She also conducted four separate community surveys, utilizing some 816 questionnaires. Along the way, she surveyed in great depth fifteen major Polish and Polish-American organizations, including many of the flagship fraternals. Oral history interviews of major participants in this drama balance her extensive bibliography of secondary sources. To her credit, Erdmans capably assists the reader with concise chapter summaries of major issues which go well beyond the scope of this review. Moreover, her final chapter on "Migrations and Generations" contains insights well worth the effort of anyone interested in the field of international migrations and ethnic studies in general, as well as Polish studies in particular. This is certainly a competent and well-researched study of recent Chicago Polonia, one that Thomas and Znaniecki would most assuredly have endorsed.

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CARIBBEAN AND LATIN AMERICA

RENÉ DE LA PEDRAJA. *Oil and Coffee: Latin American Merchant Shipping from the Imperial Era to the 1950s*. (Contributions in Economics and Economic History, number 206.) Westport, Conn.: Greenwood. 1998. Pp. xii, 191. \$59.95.

One would have thought, given the geographical location of Latin America, that shipping and a shipping industry would have been a natural development, but as René De La Pedraja, very convincingly demonstrates, that was not to be the case at all. The emergence of a merchant marine was a most difficult struggle in which not all Latin American countries even participated.

De La Pedraja has done a tremendous amount of research regarding the shipping industry with his previous work, *Historical Dictionary of the U.S. Merchant Marine and Shipping Industry* (1994), and his follow-up work to this volume, *Latin American Merchant Shipping in the Age of Global Competition* (1999). With this much research, it is very difficult to argue contrary to any of his conclusions.

De La Pedraja provides a chronological coverage of the shipping industry in the selected countries of Brazil, Mexico, Chile, Argentina, and, to a limited degree, Paraguay, Cuba, the Dominican Republic, and Nicaragua. Primarily he looks at the major companies that emerged in these countries, but he also covers the governmental and foreign involvement that affected the development of the Latin American shipping industry. De La Pedraja concludes that, prior to World War I, virtually all attempts at establishing any kind of merchant marine within any Latin American country were futile. World War I had the first major influence regarding any kind of success that Latin American shipping was to experience. Unfortunately, the interwar years once again saw a decline as Brazil, Mexico, Peru, and Chile all proved incapable of taking advantage of gains made during World War I. With the outbreak of World War II, however, Latin American shipping began to experience a renaissance, especially regarding oil shipments. But even with this tremendous stimulus, the shipping industries of Latin America experienced a decline following World War II.

De La Pedraja concludes that, "If you want to do well in Latin American shipping, 'carry only oil and coffee' might be the most practical advice from this book" (P. 165). This, of course, in no way guaranteed success at all. De La Pedraja does give a good summary of the specific problems that faced the various shipping companies of Latin America: a lack of adequate personnel, including both seaman and officers; a lack of managerial talent to handle large corporations; a lack of technology and technical ability regarding the ships themselves; a lack of facilities to construct ocean-going or even coast-wise vessels; a lack of repair facilities for those vessels that they did have; and a continuous shortage of capital investment for support of the merchant marine industry.

This book does present instructors in Latin American history with some very good economic information: basically, however, such information can be gained by reading the summary of each chapter. Throughout the text, the author provides thorough coverage of the various shipping companies that attempted to develop in Latin America. For general Latin Americanists, it makes for rather laborious reading. Likewise, for the scholar looking for new insights, De La Pedraja really does nothing more than use secondary resources to draw his conclusions; little in the way of original documentation is used. Also disappointing is the fact that the work is devoid of any statistical information, such as the number of vessels each country or company had, the total cargo tonnage that was available, etc. This kind of statistical information would have added much to the author's conclusions. I would reaffirm however, that De La Pedraja's summaries do offer some interesting ideas, and for that reason the book is worth reading.

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MARTHA K. HUGGINS. *Political Policing: The United States and Latin America*. Durham, N.C.: Duke University Press. 1998. Pp. xxi, 247. Cloth \$49.95, paper \$17.95.

Among the lacunae in the historiography of Latin America's relations with the United States is that class of Cold War-era "development assistance" that was aimed at modernizing the police forces of the region. Sired in the 1950s, adopted by the U.S. Agency for International Development (U.S. AID) in the 1960s when it was called the Office of Public Safety, liquidated by Congress in 1974 owing to its complicity in torture and murder but revived under more discreet auspices in the 1980s, the U.S. police-assistance program has bolstered the governments of at least fifty countries worldwide over the last half century. But police aid, unlike its mammoth sibling, military assistance, has attracted scant scholarly interest, even though like military aid it exemplifies one of the least-understood aspects of the Cold War: the internationalization of state-sponsored violence, particularly in what was then called the Third World. In Latin America alone through the 1960s, twenty-three governments received more than forty million dollars in equipment and services and had nearly 365,000 police officers trained by U.S. experts.

While alluring, the topic poses two obvious hazards to the researcher: government secrecy, which still blocks access to much of the evidence, and the concomitant risk of imputing either too little or too much to the U.S. role in the construction of Latin America's police forces during the Cold War. Martha K. Huggins not only fails to navigate successfully around either hazard but has produced a book that contributes inappreciably to our knowledge of this subject, either as sociology or history. While it underexploits sociol-

ogy's rich theoretical possibilities, the book's application of the historical method is so maladroit that its claim to be "reconstructing 80 years of history" should not be taken seriously.

Drawing heavily on secondary sources and mistitled as a study of police aid to Latin America, the book appears to make Brazil between 1964 and 1970—the main target of the author's research in primary sources—stand for the whole region. Yet even for Brazil, despite references to the testimony of numerous unidentified interviewees and U.S. archival records, Huggins never quite succeeds in doing more than belaboring what has been uncontroversial for at least thirty years: that the United States supplied material support for the Brazilian police even as they tortured and murdered suspected opponents of the military dictatorship. It was precisely this knowledge that helped to convince Congress to abolish the Office of Public Safety in 1974. Although the book offers some fresh detail on the mentality of the torturer and the sadism of the Brazilian police, it contributes minimally to our knowledge of their relationship with the U.S. government.

In the last paragraph of the book, Huggins seems to acknowledge some disappointment about the outcome of her efforts: "Rather than proposing a set of answers, this study has suggested a number of unsettling questions" (p. 203) that stem, she says, from the book's four "theoretical foundations" (p. 22). But the questions that it has taken 203 pages to formulate are as nebulous as her four "theoretical foundations," which Huggins identifies as follows: one of the ways "for a country to gain political control over another state is to penetrate the other country's police system," which is what the United States did in Latin America, in order to bolster U.S. national security. The penetrating power (i.e. the United States) always seeks to centralize police organizations in the penetrated countries, to make it easier to control those organizations. The result of centralization in the penetrated power is the "authoritarianizing" of internal security. No matter how successfully the police forces may be centralized, however, they inevitably pass through a final stage of "devolution" as those forces decentralize themselves again—sometimes in the form of death squads—in response to "internal tensions, social conflicts, and public resistance." Huggins argues that these four "foundations"—evidently a general model devised to account for U.S. police collaboration in all of Latin America, if not the rest of the world—emerged from the data she uncovered and led her to her main conclusion, namely, that "the fundamental purpose for U.S. assistance to Latin American police was to serve as a mechanism for gaining political control over recipient countries' internal security systems rather than to further the spread of democracy" (p. 19). On the one hand, it is hard to believe that any student of U.S. foreign policy during the Cold War could entertain the possibility that spreading democracy was ever a serious objective of the United States. On the other

hand, the idea that the purpose of U.S. police aid—much less its actual outcome—could have been that of “gaining control” of the internal security systems of twenty-three Latin American governments would attribute to U.S. policy makers a level of self-delusive ambition that probably not even they deserve. The implication—repeatedly suggested but never demonstrated in the case of Brazil—that the United States was in fact steadily “gaining control” of foreign police forces fantastically overstates the complaisance, if not the stupidity, of the governments that received U.S. police aid.

Huggins provides no evidence that the United States ever even sought “control” of Brazil’s police system, let alone that it acquired such control; nor does she demonstrate Washington’s connection with what she calls “devolution.” That the United States tried to institutionalize its influence over military-dominated or controlled governments, like those of Brazil, by supplying free or cheap equipment and services to the military and police was a frequently and publicly acknowledged goal of U.S. “security assistance” to Latin America. It may be easy to agree with Huggins that a policy of supplying repression to such bloody-handed governments was repulsive and indefensible. But she squanders the reader’s natural sympathy for her position by failing to provide either an empirical analysis or an interpretation worthy of this important but still-obscure subject.

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LUIS MARTÍNEZ-FERNÁNDEZ. *Fighting Slavery in the Caribbean: The Life and Times of a British Family in Nineteenth-Century Havana*. (Latin American Realities.) Armonk, N.Y.: M. E. Sharpe. 1998. Pp. xiv, 200.

According to the nineteenth-century German scientist and traveler, Alexander von Humboldt, Spanish laws in Cuba permitted a high degree of manumission, making Cuban slavery flexible and endowing slaves with extraordinary rights (*The Island of Cuba* [1856]). Cuba’s complex social and economic dynamics relegated African slaves to the bottom of the hierarchy, but many freed and enslaved Africans were able to attain various advantages in the system depending on their skills or personal relationships. The island’s growing plantation base and service economy created a high demand for laborers, and the demand for slaves outstripped supply. Many land holders turned to Mexicans, indigenous populations, and Asian indentured servants. Some rented slaves from others or relied on the services of the small but growing free population of color. By mid-century, the incentive for the trafficking of African slaves remained great. Havana, Cuba’s capital, had become a bustling multicultural, multi-ethnic port city with a growing population, although as late as the 1840s, slaves accounted for more than forty percent of the island’s total population.

Although Cuba did not abolish slavery until 1886, by

the second decade of the nineteenth century, slavery was under attack in various quarters, and the Spanish declared the slave trade illegal in 1820. Opposition to slavery in Cuba took many forms, from slave rebellions and sabotage to international treaties to reduce the sale of human cargo. The British, who had abolished slavery early in the century, played an important role in urging the Cuban economy toward free labor through a number of forums including the Anglo-Spanish treaties of 1817 and 1835 and, more specifically, the jointly staffed Spanish Mixed Commission for the Suppression of the Slave Trade in Sierra Leone and Havana.

This well-written book by Luis Martínez-Fernández recounts the story of George Canning Backhouse, who was sent to Cuba by the British government to serve as the British judge on the Mixed Commission in Havana. Backhouse traveled to the island in 1853 with his wife, Alice Grace Catherine, and their young son of six months; the family would remain there until 1855. The Backhouses left behind a rich trail of documents, including official papers, letters, and diaries, which constitute the major primary sources used to construct the narrative.

Martínez-Fernández provides readers with a fascinating look into life in nineteenth-century Cuba from the perspective of these British expatriates. The Backhouses’ views on life in Havana are filled with stereotypical European comments of life in the tropics, but they also reveal important insights into Cuban lifestyles, social relations, economics, and culture that makes this an important book.

This work is not about “fighting slavery in the Caribbean,” as the title suggests, however. Nor is it necessarily about fighting slavery in Cuba. Indeed, there is little information on Backhouse’s actual battles or the day-to-day duties of an antislavery judge. Rather, the British family’s views of Cuban life in the middle of the nineteenth century provide a window unto the broader political, cultural, and social climate, presenting details of everyday life for many sectors of the Cuban population, as well as some of the expatriates who found themselves in Cuba. Details of George’s hectic and troubled life and his eventual death on the island also reveal much about the inadequacies of British and Cuban institutions.

The author includes many illustrations, photographs, and drawings of streets, plazas, homes, and individuals from Cuba. Many provide helpful visual images for references, but others such as the photograph of the “slave posing in stocks” (p. 44) or the women peering through the bars from a residential property (p. 72) do not necessarily contribute to the narrative. The reproduction of the print of a *lianigo*, a member of an Afro-Cuban secret society, in the discussion of George’s death (p. 147) also seems out of place particularly since the author dismisses the claim that the society might have had a role in that death. Furthermore, none of the illustrations are dated, and

many of the references to them are not included in the bibliography.

Illustrations aside, Martínez-Fernández offers a fresh, compelling narrative that will be important (and interesting) to students of all disciplines. The narrative, which is divided into eight discreet chapters, is so well conceived that by the end of the narrative, we care about the historical characters. The author provides readers with detailed descriptions of individuals who come in contact with the Backhouses, whether it is the wet nurse who is hired to feed the Backhouses' sick child while neglecting her own, George's inept British assistant, or corrupt Cuban businessmen; all come together to help us reconstruct what life was like in nineteenth-century Havana in all its glory and decadence.

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JOAN CASANOVAS. *Bread, or Bullets! Urban Labor and Spanish Colonialism in Cuba, 1850–1898*. (Pitt Latin American Series.) Pittsburgh, Pa.: University of Pittsburgh Press. 1998. Pp. xiii, 320. Cloth \$45.00, paper \$19.95.

This book examines a topic that has received little attention by historians of nineteenth-century Cuba. Not since Jean Stubbs's *Tobacco on the Periphery: A Case Study in Cuban Labour History, 1860–1958*, (1985) has there been a monograph in English on the status of free urban workers in the nineteenth century. Therefore, Joan Casanovas's study of the evolution of Cuba's labor movement during the last half of the nineteenth century, and its impact on Spanish colonialism, is a welcomed contribution to the historiography of Cuba. He has successfully expanded on Stubbs's work, reconstructing a more detailed history of how working-class Cubans, specifically tobacco workers in Havana and its suburbs, sought to improve their lives during the period in which Cuba moved from a slave society to a post-emancipation society. Unlike Stubbs, he also explores how this labor movement affected the colonial relationship as well as how the political and economic developments in Spain influenced Cuban workers.

Casanovas argues that by the end of the 1880s, most cigar makers employed in Cuba's largest urban industry, acting as the vanguard for the working class, had discarded the reformist populism developed by the 1860s to improve their lives, and appropriated a more radical ideology influenced by Spanish anarchism. Urban workers then became better organized and successful in gaining concessions from their employers as well as from the colonial authorities. The labor movement's initial successes heightened workers' expectations. Realizing that their aspirations were incompatible with the reactionary and repressive nature of Spanish colonialism, workers became supporters of the independence movement beginning in 1895.

Examining this process between 1800–1880, Casano-

vas discovered that race and nationality affected the emergence of an identifiable working class, its organization, and its subsequent effectiveness as a movement to improve the lives of Cuba's working men and women, white as well as non-white. Spanish colonialism also affected the development of a viable labor movement before 1880. In protecting African slavery and other forms of coerced labor and reinforcing the socioeconomic and political boundaries of a caste system, urban workers found themselves racially and occupationally segregated. Colonialism prompted Afro-Cubans, whites, and later Asian workers and artisans to establish their own racially and occupationally exclusive mutual aid societies for economic assistance and recreational purposes. Creole and peninsular elites established numerous cultural institutions too. The number and importance of these organizations increased as the tobacco industry expanded, as the urban work force increased, and as Cuba experienced an economic crisis during the 1850s. The elites used their associations to influence the colonial government to change its commercial policies and to reform the political system. Workers' societies served to soften the shock of unemployment. Casanovas convincingly demonstrates that the first labor organizations to emerge by the early 1860s did so out of these elite-worker-artisan benevolent associations.

The first labor organizations of the 1860s were informed by reformist populism. This ideology advanced the notion that Cuban workers had the right to establish cooperatives and trade unions, to bargain collectively, to elect delegates for this reason, and to gain an education. Some populists went further and introduced the socialism of Charles Fourier and others into their platform as well as discussing the germaneness of the international labor movement to Cuban workers. By 1868 and the first War for Cuban Independence, white and black artisans had responded to the populism of such reformists as Saturnino Martínez.

The Ten Years' War (1868–1878) also helped the labor movement to evolve. Thousands of workers left Cuba for the United States as victims of a paranoid and reactionary colonial government that attacked supporters of reformist populism and independence. In the U.S., they used their associations to fund the separatist cause not only at this time but also during the postwar period, 1878–1895.

The Treaty of Zanjón that ended the war created a liberal social and political atmosphere that encouraged the spread of collectivist-anarchism and its strategies of class struggle between 1882–1886. Casanovas's examination of this period is the strength of the book. Employing a rich and diverse collection of archival material from Spain, Cuba, and the United States, Casanovas meticulously intersects the critical events and developments surrounding the anarchist and communist labor movements in Spain with the Cuban movement. More importantly, he demonstrates that Cubans became very selective in this process. Enrique Roig San Martín and others guided Cuban workers to

construct their own type of anarchism by taking from the Spanish movements only those principles, organizational models, and strategies that could be successfully applied within colonial Cuba. Members of the popular classes became attracted to anarchism because it afforded them more options to improve their working conditions and wages than reformist populism.

Cuban anarchism increased the number of successful strikes and collective-bargaining agreements in many urban trades and industries. It also prompted colonial officials during the first half of the 1890s to stifle the movement at a time when the economy suffered from both Spanish and North American commercial policies resulting in massive unemployment. By 1895, these developments compelled Cuban workers to support the independence movement now led by José Martí.

This book takes a new approach to the study of the evolution of the Cuban labor movement after 1850. Casanovas's thoroughly researched study adds significantly to the literature on the relationship between African slaves and free urban workers before abolition, what socioeconomic and political conditions led workers to appropriate specific ideologies and strategies to improve their lives, and to what extent this sector of the popular classes assisted in transforming the colonial state. The study is most insightful when Casanovas converges the evolution of the labor movement with Spain's political developments and its colonial relationship with Cuba.

Yet, Casanovas's study is incomplete in some areas. His examination of the origins and importance of the Afro-Cuban *cabildos* and mutual aid societies and their influence on the urban popular classes and colonialism, before and after 1850, is incomplete. Nor does he illuminate the role played by Afro-Cubans, and the separatist movement in general, in forcing Spain to address the question of the abolition of slavery and the issue of reforming the colonial relationship during the Ten Years' War. Finally, his discussion of the labor movement's commitment to racial equality needed to be better demonstrated and documented to show that it fully supported Martí's ideas and efforts to construct a racial democracy in Cuba. Nevertheless, this is an insightful study that ought to become recommended reading for undergraduate and graduate courses on Latin American and Caribbean social and labor history as well as courses on colonialism.

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G. POPE ATKINS and LARMAN C. WILSON. *The Dominican Republic and the United States: From Imperialism to Transnationalism*. (The United States and the Americas.) Athens: University of Georgia Press. 1998. Pp. xiv, 293. Cloth \$50.00, paper \$20.00.

There are only a handful of countries in the world where it is possible to state that the United States has

so influenced political development and guided economic destiny that national sovereignty was compromised. The Dominican Republic is one of those countries. From its near annexation during the Grant administration to the various administrative and military interventions initiated by Theodore Roosevelt, Woodrow Wilson, and Lyndon B. Johnson to the substantial trade, aid, and investment ties that have redefined the modern economy, the involvement of the United States in Dominican national life has been substantial and crucial.

Because the imprint of the United States is clearly visible in the Dominican Republic, it is a worthy topic for scholars of American foreign policy, Caribbean politics, and intervention. Thankfully, two of the most capable scholars in these areas, G. Pope Atkins and Larman C. Wilson, have taken up the task of describing this complex and confounding linkage between the great Colossus of the North and the nation that Christopher Columbus called Little Spain. The result is a thorough and absorbing study that details the range of involvement by U.S. presidents and policy makers and the impact of that involvement on the Dominican Republic.

The strength of the book is in its historical analysis. Atkins and Wilson are well versed in the evolution of the Dominican Republic, particularly the key period during the Trujillo dictatorship (1930–1961), when the United States courted the Dominican strongman in order to ensure political stability and regional security. The authors provide a number of insider stories of U.S. ties to Rafael Trujillo and of the clandestine efforts to have Trujillo removed from power once he became a human rights embarrassment. After Trujillo was assassinated, Atkins and Wilson discuss the rollercoaster ride of democratic development that took the country from the short-lived presidency of Juan Bosch to the tragic civil war and American intervention in 1965 to the authoritarian-like democracy of Joaquín Balaguer after the U.S. brokered the peace and left.

Atkins and Wilson correctly point out that U.S.-Dominican relations in the contemporary period have changed dramatically from the era of blatant imperialism. Since the end of the civil war, the involvement of the United States in Dominican national life has been limited to massive infusions of public and private capital to jump start the economy and occasional pressure to guarantee that elections be free and fair. The authors describe how U.S. pressure on the Balaguer government during and after the 1978 and 1994 elections transformed the political landscape. Yet despite these ventures into domestic politics, there was great reluctance on the part of the United States to become entangled in Dominican affairs. Rather, Washington increasingly relied on dollars and the withholding of dollars as the stimuli for development and democracy.

As the imperialist approach faded, the relationship between the two countries matured; the United States

avoided overt intervention, and the Dominican Republic sought to expand its diplomatic ties and intensify its efforts to attract tourism, light assembly export industries, and offshore financial accounts. Today the relationship is best described as transnational, with the public presence of the United States greatly reduced, while private investors in the export processing zones and a whole range of nongovernmental aid organizations replace the once-dominant American embassy as the center of foreign influence in the Dominican Republic.

The crucial transition was the election of Leonel Fernandez to the presidency in 1996. During his administration, the Dominican Republic began its break with the old regime, introduced a new generation of political and economic leaders, and embraced a more global approach to national development. Although politically Fernandez has had his troubles, the Dominican economy is humming along at a record pace and the country is enjoying a period of relative prosperity. As for the United States, it has blended into the background on political matters and contented itself with assisting the Fernandez government with its trade and investment initiatives.

Because the Fernandez government has been at the center of this transformation of internal politics and external relations, it is disappointing that Atkins and Wilson gloss over this most recent period. The study would have been strengthened if it examined more closely the ways in which the United States is working with the Dominicans in this new global environment. Although the authors describe in general terms the new transnationalism, the specifics of how this transnationalism is being applied in the Dominican Republic and how it has transformed U.S.-Dominican relations are unfortunately absent. This is perhaps the topic for another book. Nevertheless, Atkins and Wilson should be credited with writing a valuable study of U.S.-Dominican relations. They have fashioned a book that chronicles a remarkable series of linkages that for better or worse have left an indelible mark on the body politic of this Caribbean nation.

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CARL C. CAMPBELL. *The Young Colonials: A Social History of Education in Trinidad and Tobago, 1834–1939*. Kingston, Jamaica: University of the West Indies, 1996. Pp. 387. \$20.00.

This book is a comprehensive, richly textured study of the evolution of educational policy in the Crown Colony of Trinidad and Tobago over the century bracketed by the abolition of slavery in the British Empire and the outbreak of World War II. Carl C. Campbell is well equipped for his task and well credentialed: his publications include *Colony and Nation: A Short History of Education in Trinidad and Tobago* (1992); *Cedulants and Capitulants: The Politics of the Coloured Opposition in the Slave Society of*

Trinidad (1992); and articles on Jamaican and Trinidadian social history in the nineteenth and twentieth centuries. It is Campbell's intention to follow up this book with a study of educational policy in Trinidad and Tobago in the postwar decolonization and independence periods.

Just about every social factor that bore on educational policy in colonial Trinidad and Tobago in the period 1834–1939 is examined here: race and color; class and ethnicity; gender and religion; and the separatist proclivities of Tobago, the smaller, less populous of the two small islands comprising Britain's Crown Colony at the southern end of the Caribbean archipelago. All of these factors are rightly seen by Campbell as weighing heavily on issues of educational policy and upward social mobility in a heterogeneous, fragmented society dominated by a small minority of whites and mixed-race elements increasingly challenged by the majority of color—freed slaves of African descent and their progeny and successive generations of East Indians who had been brought to Trinidad and Tobago over seven decades, beginning in the 1840s, to be indentured laborers, mostly in agriculture. Campbell highlights the important role played by teachers, trade unions, and the Hindu and Muslim communities in the formulation of educational policies for the Crown Colony, along with the social history of various secondary schools and the role they played in educating a remarkable group of nonwhite youths who were destined to rise to great heights in the academic, intellectual, political, and governmental spheres (for example, C. L. R. James and Eric Williams).

Of especial interest in the colonial history of Trinidad and Tobago, a society marked by great religious diversity and rivalry, is the issue of denominationalism in education. The struggle over denominationalism in the school curriculum intensified in the mid-nineteenth century. Just as Indian immigration was beginning to build, in 1851 the governor, Lord Harris, moved boldly to effect education reform. Under the new scheme, an extensive system of public primary education would be funded by local rates and would be accessible to all children; provision was made also for a government-appointed Board of Education to oversee the professional staff and a wholly secular curriculum. Not surprisingly, this initiative by government aroused bitter opposition from the Roman Catholic Church—especially from its members in the French community—and from other Christian denominations. The churches' long struggle against Harris's reform was capped by success: in 1870, a new Education Ordinance was enacted that allowed the various denominations to reassert their dominance over education in the Crown Colony, aided by a rising level of public subsidy. In that way, new life was injected into the old practice of integrating education with Christian missionary activity in an extensive system of denominational primary schools. Passage of the ordinance that year, however, did not end the struggle in Trinidad and Tobago between church and state over

ultimate authority in education. That struggle endured until the twilight of the colonial era nearly ninety years later, with resonances in the postcolonial era beginning in the 1960s. New initiatives undertaken during the long administration of Prime Minister Eric Williams would, by his death in 1981, endow independent Trinidad and Tobago with the most impressive educational establishment in the anglophone Caribbean—albeit, as Campbell points out, one beset by staggering problems. Doubtless, in his follow-up book he will subject those problems to the same careful examination that is the hallmark of this volume.

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MATTHEW RESTALL. *Maya Conquistador*. Boston, Mass.: Beacon. 1998. Pp. xvi, 254. \$25.00.

This book belongs to the historiography arguing for greater use of native-language, rather than Spanish, sources in the study of the indigenous peoples of America. Matthew Restall makes a significant contribution to the scholarship about the Maya. His book consists of early colonial primary sources translated from Yucatec Maya to English (with the exception of one document written in Spanish by a Maya). Restall did the translations himself and also wrote two introductory chapters and excellent commentaries on each section of documents. The purpose of the book is to provide English readers with materials to gain knowledge and understanding of the Maya of colonial Yucatan.

The sources are organized thematically into eight chapters. Different genres of documents are represented, including primordial titles, community and lineage chronicles, the community histories/prophecies known as the Chilam Balam, a report from the famous *relaciones geográficas*, letters written by village governments to the king of Spain, and petitions by village governments or nobles complaining about local abuses.

This is philological scholarship at its best. Restall guides the reader through each reading by indicating in the notes where several interpretations are possible and where his interpretation of the text either differs from those of previous scholars or is based on good guesswork. The notes also provide brief histories of each manuscript, which in themselves make for interesting reading. One can only come away with the feeling that Restall is a master of his craft.

At the same time, Restall knows the limits of what he has accomplished. He does not claim that the documents are somehow “pure” manifestations of Mayan culture. As he points out, the sources are the product of the colonial era: they were written using a Latin alphabet—taught to the Maya by the Spaniards—and drawn up for reasons relating to the colonial regime, such as to prove a family’s or lineage group’s performance of services to the Spanish king, to

defend the exercise of political power in specific locales, to assert property ownership, or to get special treatment or favors from the crown. They clearly are not examples of a straight-forward process of maintaining Maya cultural patterns long after the conquest.

Moreover, Restall, unlike some of his advocates (who wrote the dust cover “blurbs”), does not claim to have discovered or presented “the” Mayan perspective on the Spanish Conquest. Rather, he presents several of the many Mayan perspectives on the conquest. The sample, as he recognizes, is not representative. With few exceptions, the points of view given are those of the leaders, lineages, and communities that sided with the Spaniards against other Mayan groups at the time of the Spanish invasion. That is why some of them called themselves “Maya conquistadors”—hence the title of the book—for in fact they fought on the side of the Spanish conquistadors. Unfortunately, little documentation from the losing side—especially the Maya peoples of central and eastern Yucatán—has survived to be translated to give a less contented perspective.

Furthermore, as Restall notes, the focus of the Mayan writers is intensely-local, clannish, or familial in nature. These documents were drawn up by people who saw themselves as members of specific communities or lineage groups, not as an oppressed, conquered people. They would never have dreamed of presenting a “Maya” perspective of anything. However, their writings do provide good insight into Maya culture. Especially noteworthy are the importance of lineage and ancestry and the Maya passion for emphasizing how the past determines both the present and the future. The resulting documents, by the way, do not always make for exciting reading, for genealogies are frequently uninteresting to outsiders, and Mayan patterns of thought are so different from those of modern readers that meaning and significance are often unclear or enigmatic.

This does not, of course, detract from the accomplishment of Restall, who must be credited with producing another excellent book on the Maya of Yucatán. It might also be noted that although Restall does not address historiographical issues in this book, it does manifest the guiding spirit of Restall’s mentor, James Lockhart. The book is modeled, consciously or unconsciously, on the latter’s monograph, *Letters and People of the Spanish Indies* (1976), a translation of documents from Spanish to English accompanied by commentaries at once erudite and didactic. With this, his second book on the Maya of Yucatán, Restall now surely qualifies as one of Lockhart’s most accomplished students.

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TIMOTHY J. HENDERSON. *The Worm in the Wheat: Rosalie Evans and Agrarian Struggle in the Puebla-Tlaxcala Valley of Mexico, 1906–1927*. Durham, N.C.:

Duke University Press. 1998. Pp. viii, 2888. Cloth \$54.95, paper \$18.95.

This tale about the clash between two cultures begins when a Texas gentlewoman took up the challenge to protect her hacienda from seizure by the agrarians during the Mexican Revolution of 1910. Timothy J. Henderson develops beautifully this Manichean struggle between two mentalities and two ways of life for control over an estate that both sides considered rightfully theirs. This meticulously researched monograph, which reads like a novel, is essentially premised on the view that agrarian reform does not bring about the redistribution of power and wealth but rather wealth, social standing, and political power determine who can exploit the land profitably. In taking this position, Henderson joins a growing number of scholars who are attacking the heroic image of Mexican agrarianism and questioning the populist interpretation of the Mexican Revolution espoused by Alan Knight, Gilbert Joseph, and others.

Rosalie Evans's private correspondence and publications are the principal sources for this incisive biography of a sedate, astute, intelligent, bigoted, and at times compassionate woman, luxuriating in the privileged society enjoyed by foreign residents of Mexico before the revolution. She became caught up in the chaotic revolutionary strife of the Puebla-Tlaxcala Valley, however and was transformed into an "outlaw" or "rebel" in her defense of private property. She and her husband, a British banker, shared the contemporary Anglo-Saxon mentality toward a "backward" country and perceived themselves as the bearers of an enlightened, civilized way of life and the initiators of progressive, capitalist agriculture. She regarded the "Indians" as childlike, lazy creatures who were in need of her compassion, discipline, and guidance. The Evans' tranquil life was disrupted by the seizures of land carried out by Emiliano Zapata's and Domingo Arenas's forces in the Puebla-Tlaxcala Valley in 1911. Rather than selling their wheat-producing hacienda outright or dividing it up into parcels among family members, as other landowners in the region had done, they chose to stand and fight. After the death of her husband in 1917, Evans became transformed by her crusade to save what she considered rightfully hers. She was Machiavellian in her use of legal, political, diplomatic, and gendered strategies to appeal to the Mexican villagers, local *caciques*, military commanders, governors, cabinet ministers, and even the president. When her legal battles against expropriation failed, she obtained an interview with President Alvaro Obregón and scolded him for robbing a helpless widow of her lands. This audacious tactic backfired, so she next turned to the British *chargé d'affaire*, who transformed her legal battle into an international cause resulting in a diplomatic disaster. Finally, Evans wooed the U.S. media, which was all too willing to publish her interviews and letters criticizing the Mexican government for harassing U.S. citizens and seizing their

properties. When all else failed, she holed up with a hired Texan sharpshooter in the hacienda and tried to run the villagers off her land with firepower. Her strategies could only lead to one conclusion: her assassination at the hands of the agrarians in 1924.

Henderson's portraits of the Mexican agrarians, who are on the other side of this conflict, are all quite unsavory. From the local *caciques*, Manuel P. Montes and Arenas, to the state governors and the president himself, these revolutionaries seem to share in common a propensity for employing whatever means available to gain political power. In fact, Henderson takes a dim view of the Mexican agrarian leaders in general and the villagers themselves, whom he considers to be the pawns of the warring leaders in their struggle for power. Arenas is characterized as not much more than a common bandit, shifting his allegiance from the Zapatistas to the Constitutionalists, while Montes is a violent *cacique*, willing to massacre some peasants in order to intimidate others into submission. The long list of governors who attempted to restore order in Puebla in the early 1920s had little or no control over the countryside and thus became the puppets of Presidents Obregón and Plutarco Calles, who sought to centralize power in their own hands at the expense of the states.

In the Puebla-Tlaxcala region, which had a long radical agrarian tradition, Henderson contends that the Mexican agrarian reform process failed to fulfill its goals of bringing social justice and economic equality to the countryside. There is no doubt that the peasants did not receive sufficient land to become self-sustaining farmers without further financial and technical assistance from the state. Whether it is possible to generalize for all of Mexico based on this particular case is still open to debate. One might also ask whether it is possible to label agrarian reform in the Puebla-Tlaxcala valley a failure without a thorough analysis of the Cárdenas period. Henderson's argument would have been strengthened if he had elaborated on the successes and/or failures of the military and ejidal grants in this region.

This fine monograph should be read as a study of personalities involved in the politics of agrarian reform in a multileveled interpretation of the Mexican Revolution. It brilliantly elucidates the mentality of the foreign landed elites in decline and their inability to adapt to a changing Mexican reality. Henderson's categorization of agrarian reform as "probably no reform at all, but more probably a spontaneous jacquerie of little lasting consequence" (p. 1) seems to underestimate the long-term effect of postrevolutionary reforms on land tenure patterns, agriculture, and ejidal decision-making structures. This, however is one of the mostly hotly debated issues of the Mexican Revolution.

HEATHER FOWLER-SALAMINI
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ANNE RUBENSTEIN. *Bad Language, Naked Ladies, and Other Threats to the Nation: A Political History of Comic Books in Mexico*. Durham, N.C.: Duke University Press. 1998. Pp. x, 210. Cloth \$49.95, paper \$17.95.

This book by Anne Rubenstein constitutes a proficient study of comic books (*historietas*) and other kinds of popular literature (such as *fotonovelas*) in Mexico (indeed Mexico City) from the 1930s to the 1970s. It combines a conventional chronology of postrevolutionary politics with an engaging conceptual approach to the formation of both a national culture and a Mexican state. The author uses *historietas* as means to inspect the networks of mediation between, and within, a "civil society" and a state apparatus. In so doing, Rubenstein furnishes an insightful account of postrevolutionary Mexican "cultural wars." Her examination is based on a broad sample of comic books, and, more importantly, on the archive of the Comisión calificadora de periódicos y revistas—a state censor—where the content and form of popular media were ventilated. This is a story, the author professes, with a happy ending. Out of comic books, "the people" obtained what they sought, publishers and the media profited, and the state discovered its own identity and indispensability in the mediation of long-lasting cultural conflicts.

Conceptually, the book draws on Alan Knight's notion of change and uniqueness in Mexican history from the 1880s to the 1940s. It projects the theme of Mexico's particular political and cultural stability toward a "modernity-tradition" scheme within the framework of global capitalism and nation building. In such a context, as the author maintains, "'tradition' did not precede 'modernity' any more than modernity displaced tradition. Each required the other. And both were aspects of a single national culture that was developing throughout this period" (p. 42). Rubenstein expends great effort (for this reviewer, to little effect) to dispute the usefulness of Gramscian "hegemony" (as used in English-speaking academic circles). It would be wise to let "hegemonologists" reason with Rubenstein on this point.

Chapters are organized chronologically and thematically. The most engaging sections are those dealing with the quarrels between opposing understandings of universal values and the nation's social and cultural standards as they were aired in the Comisión Calificadora. Rubenstein skillfully reads specific *historietas* to interpret their gender, racial, and national connotations. Yet she shows more convincingly the process of quasi-chaotic arbitration and mutual interaction among the potential public, readers, and social groups (the church, conservative organizations, and official agencies) involved in the production and consumption of this popular literature.

In terms of the fabrication of gender and "modernity" stereotypes, the book focuses on the figure of *la chica moderna* (the modern woman) as portrayed ambivalently by popular comic books. According to

Rubenstein, *la chica moderna* both fascinated readers and threatened traditional Catholic values. In fact, these Catholic values are part, in the author's view, of a larger cultural struggle. The book interprets lucidly the bargaining among three "cultures": official revolutionary statism, nationalism, and social reform; conservative tradition linked to Catholicism; and the mass culture of modern capitalist societies. By examining the interaction of these "cultures," the book tells a story of constant trial and error, adjustment, and adaptation.

As a whole, however, the book bears the same "agoraphilia" that the title unfurls. Lofty conceptual and theoretical assumptions are discussed and then tested by analyzing some *historietas*, *fotonovelas*, or *historias de amor* as if they posed a clear danger to the nation, as if there had been an open and vital national debate over obscenity. As popular as comic books were, and as insightful as Rubenstein's perspective is, one should keep in mind that these publications were not threats to the nation even if the "sons of Sánchez" read them.

The book's substance, nonetheless, is jeopardized: first, by the lack of attention to language; second, by a sort of untidy "hermeneutics"; and, finally, by Duke University Press's careless production. Rubenstein selects excellent graphic samples. But the prints are of poor quality and hard to read, and the author's treatment of them often displays an inadequate mastery of the intricacies of language—inadequate especially for a study that aims at such high theoretical altitudes. For instance, Rubenstein sees no differences between the common use of the term *de cuna humilde* and the bizarre term, very likely a typo, *de cuña humilde* (p. 64). Or take the example of a 1950s cartoon, reproduced in the book, which shows a character entering the national museum of art that happened to be exhibiting the works of the officially promoted Mexican painter, Diego Rivera. The character says, in pure Mexico City slang, "*me siento muy nervioudo*." Rubenstein labels the cartoon: "Here Gabriel Vargas abandoned his usual spare style for a detailed and exact sketch of the nation's most important art museum" (p. 105), as if somehow to suggest Vargas's respectful participation in the creation of national symbols. In fact, the language of the cartoon conveys irony through a coarse character who, we are humorously told, felt *mosqueado* and compelled to strengthen himself with some *copetines* (drinks). Tensely he enters such a grandiose building in order to defy a certain *Panzón* over the reputation of some woman. Vargas, it seems, was less interested in showing respect to the museum than in making a many-sided joke that mocked the grandiosity of the palace and the national painter vis-à-vis the ordinariness of the people. It could also have referred to the satire found in the fact that Rivera's name appears on the front of the national museum, and that the cartoon's character was looking for a certain *Panzón*, when Rivera was often mocked as the fat king of Mexican

art. This is the kind of "thick" irony lost by the book's lack of attention to language.

Notions such as modernity and tradition, as well as the idea of three different "cultures" (official, conservative, and modern capitalist), seem too vague to support the conceptual structure of this book. Rubenstein lucidly shows the contradiction and changes within the official national ideology over the years. But she finds herself very comfortable in maintaining the existence of a traditional culture—conservative, Catholic, in constant transformation and adaptation to "modernity"—which is never defined. Modern consumerism and capitalism, in turn, are simply seen as the outside forces of market and U.S. influence. As useful as these concepts are, they would have been even more profitable if considered, in good old Weberian terms, as mere ideal types and not empirical entities. Where and how could one find what the author names "Mexican conservatives"? There were too many revolutionary ideologues who were both socially revolutionary and morally conservative; there were various moral revolutionary dandies and groups that were nevertheless terribly conservative politically (Salvador Novo and *Los Contemporáneos*, for instance); there were moral conservatives who were Americanized and market oriented. And, by the 1960s, many inhabitants of Mexico City were all of these, all of the time: officially nationalists when attending a soccer match or the 1968 Olympic games, morally conservative when the virginity of their daughters was at stake, and globally aware when permitting their kids to read a cheap Mexican *historieta* instead of a sanitized translation of "Donald Duck." Finally, the book would have benefited tremendously from better reproductions of cartoons, and from a bibliography.

All in all, I reiterate my original and definitive conviction: this is an insightful and interesting book, which is likely to attract a lot of attention in the study and teaching of contemporary Mexico.

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Austin

VIRGINIA GARRARD-BURNETT. *Protestantism in Guatemala: Living in the New Jerusalem*. Austin: University of Texas Press. 1998. Pp. xvi, 248. Cloth \$30.00, paper \$14.95.

Whereas in 1940, less than one percent of Guatemalans were Protestant, and of these most belonged to a few mainstream denominations, by the early 1990s as much as a third of the population claimed to be Protestant converts, and they belonged to some 10,000 mostly evangelical and increasingly pentecostal churches. Virginia Garrard-Burnett's book makes good use of newspapers, church records, state archives, and extensive interviews to detail this dramatic shift and to discover its roots in a deteriorating economic and social situation and in the nation's ongoing political violence.

There are three standard explanations for the recent rise of Protestant Christianity in Guatemala. One argues that victims of the 1976 earthquake joined the new churches for access to relief assistance that the government and the Roman Catholic Church could or would not provide. Others claim that the conversions were an attempt to declare neutrality in the fighting between the guerrillas and the armed forces during the 1980s. And anthropologists have shown that the evangelicals' stress on temperance is attractive to the economically ambitious and entrepreneurial among the indigenous population who wish to opt out of expensive, alcohol-sodden community rituals. No doubt each of these interpretations has some truth to it, but they all miss the point that for many religion genuinely is about belief and faith. To study religion, Garrard-Burnett reminds us you do not need to believe yourself, but you do need to understand that others believe.

The first half of the book is largely a narrative of failure. Protestant missionaries entered Guatemala as early as the 1820s, and their numbers increased under the Liberal governments in the half century after 1871, but they met with little success. National leaders thought the missionaries useful as "civilizing agents" (p. 13), but the mass of the indigenous population showed little interest in the new faith, preferring a community-based folk-Catholicism (*costumbre*) developed over the centuries since the Conquest. Indeed, participation in *costumbre* defined community and self-identity, and anything that threatened this involvement threatened the integrity of both. The most serious challenges, in any event, came not from Protestantism but from the effects of the spread of agrarian capitalism and from Catholic Action, a missionary effort by the Roman Catholic Church to reintroduce orthodox ritual in the countryside.

Guatemalan Protestantism grew explosively only beginning in the late 1960s and early 1970s, and here the author does an excellent job of navigating a potentially bewildering array of sects and schisms. What accounted for this sudden upsurge? Garrard-Burnett argues that the new Protestant churches provided their members a sense of belonging that had been lost as traditional communities crumbled, undermined by a century of "modernization" and shredded by civil war. Whereas before only deviants could accept the ostracism that conversion entailed, now, as violence dismantled the communities and dispersed their populations, the new sects' pentecostal visions and premillennial beliefs helped make sense of a world in chaos. With a Liberation Theology-infected Catholic Church largely hounded out of the highlands by the military and with populations from various ethnic and cultural backgrounds thrown together in the new "development poles" and "model villages," Protestant churches were among the few relatively safe vehicles available for association.

These churches usually, but not always, offered a refuge from government violence both because they

were perceived to be linked to notoriously conservative North American groups supportive of the military and because most urged their members to eschew activist politics. The Left routinely accused the evangelicals of cooperating with the military, but Garrard-Burnett finds that where this occurred it was less the result of ideological convergence than of fear and intimidation. Over time, the new churches tended to escape foreign tutelage. Some began to reincorporate elements from *costumbre* into worship, and their members found that they could not escape local social and political realities.

This is a well-researched and well-written book that will be valuable to scholars and useful, too, for the classroom. There are a few minor errors: for example, it is unlikely that Guatemala ever exported 51 million tons (as opposed to pounds) of coffee (p. 10), and Jorge Ubico's 1934 labor law abolished long-term debt peonage, not *mandamientos* (p. 73). It would be interesting, too, to learn more about interactions between the Protestant churches and the civil patrols that formed the chief alternative institution claiming loyalty in highland villages during the 1980s. But these are minor points and in no way detract from the value of Garrard-Burnett's work, which is a fine addition to the growing literature on the history of religion in Central America.

DAVID MCCREERY
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JAMES HOWE. *A People Who Would Not Kneel: Panama, the United States and the San Blas Kuna*. (Smithsonian Series in Ethnography Inquiry.) Washington, D.C.: Smithsonian Institution. 1998. Pp. x, 390. Cloth \$55.00, paper \$19.95.

Many North Americans and Europeans probably know the Kuna peoples mainly through their colorful and now commercialized mola fabrics, a derivative of reverse-appliqué blouses. Long a centerpiece of traditional women's dress, those mola blouses were also part of an ongoing struggle between the Tule-speaking Kuna, in recent centuries centered in Panama's San Blas Islands and adjacent mainland coasts, and various forces that sought to dominate and/or change them; given that "women's dress symbolized . . . the ethnic identity of the Kuna and their separation from national society . . . an attack on women's dress was merely the opening battle of a general war against all of indigenous culture" (p. 125).

Over time, the Kuna were caught up in a range of Isthmian struggles, occasionally dividing their communities but more often both threatening their independence and providing them the means to counter their enemies, usually outsiders derisively called *wagas*. Between two chapters of background and one of follow-up (out of twenty-eight total), anthropologist James Howe focuses on the years 1903–1925 to illustrate sympathetically how outside pressures impinged on the Kuna, how the Kuna reacted, and how they

managed to retain a high degree of autonomy. Across these two decades, on balance, the Kuna, though not always in agreement among themselves, were more likely to opt for the action that most distanced them from the new Republic of Panama: residual loyalty to Colombia and a refusal to fly the Panamanian flag; friendship with North Americans; Protestantism over Catholicism, though their own religion was preferred; some form of English over pure Spanish; and resistance to "modern" education and methods. They adeptly played off the *wagas* against each other from presidents to diplomats to bureaucrats to clergymen, spoke in vague and ambivalent language, and erratically changed their stance. On occasion, they used a degree of force, then covered for friends.

Ultimately frustrated and feeling threatened, the Kuna chose limited armed rebellion in February 1925, even issuing a declaration of independence and human rights. Thanks to domestic and international circumstances they won a "partial and tentative" victory (p. 292). Since then they have gradually accepted a degree of formal education, national and international integration, and Panamanian authority, but always within the context of significant cultural, economic, and political self-determination, however difficult to maintain due to continuing outside pressures and internal disagreements.

Through this study we are introduced to several interesting outsiders who represent the kinds of intrusions the Kuna repeatedly faced: Spanish Jesuit Leonardo Gassó who in laboring to Christianize the Kuna between 1907 and 1912 sought, among other reforms, to suppress excessive consumption of alcohol, especially when linked to the puberty ceremonies called *chichas*; the British evangelical Anna Coope, who likewise condemned drinking and puberty rites but who also battled Father Gassó and anything Catholic; Richard O. Marsh, discredited U.S. diplomat, pseudo-anthropologist, racist, explorer, and frustrated entrepreneur, yet vigorously devoted to the Kuna cause as he perceived it and eventually aiding the 1925 rebellion; and numerous, often inept and underpaid Panamanian policemen, more interested in local women and survival than their professional duties.

It is an exciting story, enthusiastically told. The book contains a good bibliography, although the use of sources is unbalanced; it would benefit from more Panamanian materials. It definitely needs more and better maps. In places, driven by the sources, the author includes too many details unrelated to the main narrative. Also, given the dispersion of Kuna communities, it is not always clear how representative any single Kuna leader or village is of the entire group. And, call me outdated, but I still don't comprehend the capitalization of "Blacks" and not of "whites." In sum, be it because of the story or the storyteller, you cannot help cheering for the Kuna.

JOSEPH L. ARBENA
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MARÍA ROSTWOROWSKI DE DIEZ CANSECO. *History of the Inca Realm*. Translated by HARRY B. ICELAND. New York: Cambridge University Press. 1999. Pp. x. 259. Cloth \$49.95, paper \$14.95.

In some sense, María Rostworowski de Diez Canseco's book represents a summation of her life's work, which has been dedicated to interpreting indigenous Andean society through the analysis of a diverse array of Spanish colonial period documents, including chronicle and ecclesiastical literatures, administrative reports, census records, and litigation dossiers. Rostworowski's four decades of assiduous archival research have revealed new primary source materials as well as generated fundamental analytical insights into the nature of complex social formations in the precolonial Andean world. In particular, her work has convincingly demonstrated the social and cultural importance of economic specialization keyed to distinct ecological adaptations and sustained interethnic relations among Pacific coastal populations during the pre-Hispanic past. Rostworowski's long and unfailingly provocative stream of publications on the particularities and dialectics of coastal versus highland models of native Andean political economy continue to animate and nourish ethnohistorical scholarship in the region.

This book is a synthesis of ideas and source materials that by and large have been published in other fora, so scholars already familiar with Rostworowski's work will find little new here. The book is, however, a valuable resource for non-Andeanists and readers looking for a concise précis of theories of Inca state formation and the social dynamics of territorial expansion from an ethnohistorical perspective. The first half of the text is given over to a rapid historical survey of the formation and expansion of the Inca empire (although in her preface, the author eschews the term "empire" as a Western category inappropriate to the native Andean world).

Intercalated with this historical narrative, Rostworowski identifies specific social institutions that shaped and facilitated the Inca's extraordinary construction of the largest pre-Hispanic political formation in the western hemisphere. Specifically, she singles out the deeply rooted institution of reciprocity as a critical politico-economic tool in the early formation of the Inca state, but astutely remarks that this institution subsequently became a burden and an impediment to the territorial ambitions of Inca kings. According to Rostworowski, in the initial stages of state expansion, Inca rulers established and continually renewed strategic political ties with allies and conquered populations through a prodigious flow of gifts—including various forms of luxury goods, prestige items, and women—to subject lords. These political alliances were often reaffirmed publicly in the context of rich, exquisitely ritualized banquets hosted by the Inca.

Rostworowski succinctly notes that as "the state grew, so did the number of lords who had to be satisfied" (p. 46). The Inca responded to the ever

increasing demand for gift commodities by conquering new territory, by increasing productivity in subject provinces through alienating more land and extracting additional tribute in the form of labor, and by creating new categories of "citizens" outside of the system of reciprocity who were tied directly to the Inca state and not to their autochthonous communities of origin.

The inherent instability of this system is evident: new territorial conquests generated new revenues, but also new demands from more elite clients who anticipated a flow of gifts. The Inca rulers' drive to escalate production in subject communities through increased taxation of land and labor generated deep hostility among the ethnic lords who were their erstwhile allies. Not surprisingly, many of these subject lords and their populations eventually rebelled against the Inca and readily made the (ultimately woeful) decision to ally themselves with Francisco Pizarro in his conquest of the Inca.

In the second half of the book, Rostworowski turns to a synchronic analysis of the "organizational aspects" of Inca society in which she treats various issues of social structure, class formation, and economic systems. This analysis breaks no new intellectual ground but does serve as a concise, if somewhat fragmented and incomplete, summary of Inca principles of sovereignty, class structure, and modes of production. Rostworowski's analysis is seriously weakened by its lack of recognition that systems of belief, imperial cults, and various religious ideologies and counter-ideologies also played critical roles in the formation and expansion of the Inca, as well as in the inevitable local resistance to their hegemony.

In sum, this book is an excellent Baedeker to Rostworowski's fundamental contributions with respect to the documentation and interpretation of pre-Hispanic Andean political economy. It is not, however, a comprehensive interpretation of the internal social dynamics of the Inca state.

ALAN L. KOLATA
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MICHAEL EDWARD STANFIELD. *Red Rubber, Bleeding Trees: Violence, Slavery, and Empire in Northwest Amazonia, 1850–1933*. Albuquerque: University of New Mexico Press. 1998. Pp. xvii. 270. Cloth \$60.00, paper \$21.95.

The "Putumayo scandal" of 1907–1914 focused attention on the ill-treatment of laborers who extracted rubber from trees in the Northwest Amazonia region of Colombia, Ecuador, and Peru. The late nineteenth-century demand for rubber for use in the industries of Europe and North America "opened" the upper Amazon to commodity production and, because of its profitability, to geopolitical contention. Rubber from this region and from the African Congo dominated the world trade, producing over ninety-nine percent of the exported rubber in 1906, although not without negative human and environmental consequences. Roger

Casement's 1912 Blue Book alleged that perhaps 30,000 Indians had been killed in the extraction of rubber from Amazonia, while thousands had been subjected to torture and conditions of slavery. Coincidentally, just as Casement made his report to the British Parliament, Amazonian rubber lost its global monopoly, as rubber produced on plantations in southern Asia completely replaced the "harvested" jungle rubber in a span of ten years.

The Putumayo scandal forms the backdrop to this detailed analysis of the regional impact of production of, and commerce in, rubber. Stanfield weaves two analytical themes throughout his account: the system of rubber production and the geopolitical maneuvers to dominate the region. Together, these themes enable him to "place the scandal into the larger historical context of the Putumayo, [by] stripping away the nationalism and hyperbole that have surrounded the region, [and] to illuminate how life evolved during and after the rubber boom" (p. xvii). The author opens his study with a geographic analysis of the region, an inquiry into the importance of rubber in the nineteenth century, and an overview of the indigenous peoples who provided the bulk of the labor in the rubber industry. Subsequent chapters track "pioneers" of the industry, including the Colombian Rafael Reyes and Peruvian Julio César Arana, and the different strategies pursued by Colombia, Ecuador, and Peru to dominate the region. Finally, he traces the development and impact of the scandal.

Stanfield is at his best in the analysis of the *aviamento* system of production and the geopolitics of rubber. The *aviamento* system linked the import/export houses of the port city of Iquitos to *aviadores*, who delivered imported goods to rubber collectors; they in turn either dominated lesser rubber collectors or worked directly with the laborers who bled the rubber trees to acquire the latex. Credits and debts dominated the *aviamento* system, building into it both fragility and dominance, characteristics that are well-documented in the development of the Casa Arana that controlled most of the trade. At the bottom of the system were indigenous and Barbadian laborers who were subject to considerable physical abuse and economic dependence. Transportation and commercial networks enabled the Casa Arana to achieve its preeminent position, a position carefully guarded by the Peruvian government. The importance of rubber and import exchange is well illustrated, as is the function of the rubber collectors that facilitated that exchange. More information on the role of indigenous and other labor bosses could have better illustrated the local functions of the *aviamento* system. As commodity production created the region of Northwest Amazonia, it transcended (and helped to define) national boundaries. The author painstakingly reconstructs the efforts by individuals, local political bosses, and national politicians to develop policies and strategies to control the rubber trade. Peru's successful linkage of timely investment, military presence, and commercial relationships

allowed that nation to dominate the region, a domination that was facilitated by the ineptitude of Ecuador's Liberal regimes and by Colombia's cycles of civil conflict. Equally well developed are the international dimensions of the rubber industry and the scandal itself.

Stanfield achieves most of his objectives in this well-written and well-researched book. He pays meticulous attention to the international competition surrounding the rubber industry, although more information on Brazilian patterns would have rounded out the account. His detailed ethnographic survey of the Indians of Northwest Amazonia is notable, but he offers the reader less information on how the collapse of the industry affected social and economic patterns of the region. This book makes a valuable contribution to our knowledge of the Amazon, especially at the start of commercial exploitation of the region that continues to the present.

DAVID SOWELL
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THOMAS M. COHEN. *The Fire of Tongues: Antônio Vieira and the Missionary Church in Brazil and Portugal*. Stanford: Stanford University Press. 1998. Pp. viii, 262. \$49.50.

Padre Antônio Vieira, the Portuguese Jesuit missionary, statesman, diplomat, preacher, author, and administrator, was one of the most remarkable figures of his age. He was a man of diverse talents, at times of considerable political influence, and sometimes of such bizarre millenarian ideas that he has always defied simple generalizations. There is no full biography of him in English and few good ones in any language, including his own native Portuguese, but there are some excellent monographic studies that examine various facets of his career, and Thomas M. Cohen's book must now be counted among them.

Cohen's study concentrates on Vieira the missionary and it seeks to demonstrate how Vieira's missionary experience in Portugal's vast Amazonian territory, the Maranhão, and the theory he derived from it influenced the conduct of his life and the way in which he perceived the role of Portugal and its empire within a divine plan for the salvation of the world. The book is very much in the tradition of intellectual history and it is based on a close reading and analysis of Vieira's principal texts, but it has avoided the old pitfalls of that approach through its attention to actions as well as ideas and by a sensitivity to the way in which ideas change over time. Much of this story has been told before, but Cohen's emphasis on the importance of Vieira's missionary experience provides a new perspective.

The structure of the book is simple. Cohen first describes the arrival of the Jesuits in Brazil in 1549 and their early missionary work under the direction of Manuel da Nóbrega. Cohen is not so interested in the actions of the Jesuit missionaries as he is in the theory

that lay behind these actions. He ably demonstrates the inner conflict that plagued the early church in Brazil: how to reconcile the mission to convert the indigenous peoples with the religious needs of the European settlers; and how to minister to both simultaneously. Here as elsewhere in the book, Cohen seeks key documents (in this case the *Diálogo sobre a conversão do gentio*) upon which to employ the content analysis which constitutes his primary technique for posing and resolving historical questions.

The next two chapters explore the mission field of Maranhão, where the Jesuits sought to work closely with the crown. It was here that Vieira confronted the problems of convincing the native American inhabitants of the validity of Christianity while protecting them from exploitation by Christian colonists. Resolving this contradiction was not easy, and the Jesuits were actually expelled from the regional capital São Luís in 1661 because of colonist anger at their meddling in "*coisas do sertão*," or "matters of the interior," which generally meant enslavement of Indians. Here, Cohen makes excellent use of a series of sermons of the 1650s and 1660s in which Vieira sought to convince the colonists of the error of their dealings with the Indians. Especially interesting in this regard is the Epiphany sermon of 1662 ("the most important sermon on Brazilian society that Vieira ever preached"), in which Vieira underlined the inherent contradiction between the enslavement of Indians and their conversion. Here and throughout the book, Cohen demonstrates an understanding of scripture and an ability to relate that understanding to Vieira's message.

Chapters four and five deal with Vieira's prophetic writings and the apostolic role that he felt Portugal was destined to play. Cohen argues that Vieira's arrest by the Inquisition was the primarily the result of his millenarian writings and his questioning of ecclesiastical authority rather than his support for the Portuguese New Christians, which had intensified after his visit to Amsterdam in 1648. Vieira's desire to demonstrate the way in which Christian Portugal might unite with the Jews and the converted heathens of Asia, Africa, and America as a necessary step for the establishment of a new millenarian age provided sufficient material to make his ideas suspect. But as a respected missionary, preacher, and political advisor, he was no easy target. Although convicted by the Holy Office, Vieira was eventually able to have the sentence rescinded by Rome and himself exempted from the Inquisition's authority. Cohen finishes with an examination of a series of sermons given at the end of Vieira's life in the 1680s, in which the elderly Jesuit sought once again to link the missionary vocation of the Jesuits to Portugal's millenarian destiny.

The application of a narrow focus on the life of such a multifaceted character sometimes leaves the reader wondering about other connections. The emphasis on Vieira's theological concerns rather than on his actions as a political figure during the crucial period of Portugal's reestablishment of independence (1640–

1668) will leave some readers unsatisfied, but after Cohen's fine book no one can subsequently ignore the way in which the missionary career of Vieira shaped his theology and his prophetic vision.

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EUROPE: ANCIENT AND MEDIEVAL

ERICH S. GRUEN, *Heritage and Hellenism: The Reinvention of Jewish Tradition*. (Hellenistic Culture and Society, number 30.) Berkeley and Los Angeles: University of California Press. 1998. Pp. xx, 335. \$38.00.

Erich S. Gruen surveys Jewish-Hellenistic literature from the time of Alexander's conquests to the early Roman Empire. He asks how the Jews accommodated themselves to the cultural world of the Mediterranean while remaining committed to their own heritage. The answers he unfolds are refreshingly different from prevailing judgments and are resoundingly positive. No defensive sensitivity about their beliefs and practices and no obsession with anti-Semitism characterize the Jewish stance. Hellenistic Jews are so comfortable with Greek culture that they take great liberty with it in enhancing in a very free way their own, largely biblical traditions. Gruen tells the reader about his pride in being "a liberal, secular, diaspora Jew." The picture he paints is of a people he much admires. The account he gives fits into an enduring form of history writing: an etiological story told long after the events to explain and justify a current stance.

Gruen attacks with gusto the views of other distinguished scholars in the field. Convincingly, he shows how shaky are their endeavors to apply the historical critical method to texts that are, he constantly reminds us, imaginatively creative. It is largely futile to infer the historical setting, the social circumstances, and the intended audience of any of the texts. Attempts to understand the authors' attitudes as signals to contemporary leaders, nations, and events are "strained conjectures." He is correct to claim that we know little about the composers of the works. But he himself is open to criticism when he claims to know a good deal about the audiences: namely, Jews enjoying a comfortable existence in Hellenistic society, secure in their own heritage and confident in confronting the Greek culture around them. We know almost nothing about the recipients of any ancient document.

Much of the book paraphrases the literature, and Gruen does it uncommonly well. The inevitable problem with a survey is that any wrong readings of texts dent one's confidence in the overall assessment. However one evaluates the Israelites' request of the Egyptians for jewelry and clothing, at the time when they depart from Egypt, they are not tricking the Egyptians. (The evidence suggests that a provision for departing slaves is an obligation that masters should recognize.) In Genesis 34, Dinah is not seized and raped. Shechem "took her and lay with her and humbled her." The

latter term is technical and refers to taking a woman without the proper formalities. Nor is the biblical judgment negative on Simeon and Levi's fierce reaction to the proposed forging of connubial and commercial relations between the two groups. Jacob's is, but not the narrator's (or that of the author of the Book of Deuteronomy). Gruen's evaluation of the story as found in the epic poet Theodotus is especially affected by his misreading of the original Genesis account.

Gruen's major point is that Hellenistic Jews showed great pride in their and their ancestors' accomplishments. This pride is evident in such fictions as the claims that Aeschylus, Euripides, and Menander purveyed Jewish beliefs or that the Torah was the model for the ideas of Pythagoras, Socrates, and Plato and inspired Greek music and poetry. Moses transmitted knowledge of the alphabet first to the Jews, then to the Phoenicians and the Greeks. Other fictitious claims made are that he invented ships and made many technical breakthroughs.

Gruen is surprisingly uncritical about this kind of national sense of superiority. He is at pains to emphasize that these writings were a form of entertainment for Jews. There may be something to this view, but here also his analysis is less than convincing. In assessing the mockery directed at the Gentile ruler in 1 Esdras, he pays no attention to the influence of *parrhesia*, "outspokenness," not of the legalistic variety at Athens but of a populist hue in which an inferior is at liberty to poke fun at the flaws of his superior. By not placing in the context of "free speech" the story of the three bodyguards who debate what is the strongest force in the world, Gruen misperceives how the mockery of the ruler would be congenial to a Jewish audience but repellent to a Gentile one.

Erudite, enjoyable to read, and a stimulus to further research, Gruen's book brings out well the bizarre nature of the material and provokes the reader to look more closely at the individual works. He also raises a fundamental issue: does it make much sense to seek from these ancient narratives conventional forms of historical knowledge?

CALUM CARMICHAEL
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MICHAEL RICHTER. *Ireland and Her Neighbours in the Seventh Century*. New York: St. Martin's 1999. Pp. 256. \$55.00.

The purpose of Michael Richter's book is to defend the importance of Ireland's contribution to Western learning in the seventh century against recent scholarship that questions its significance. To this end, he assembles much useful information and provides a worthwhile synthesis of the substantial amount of secondary literature on this subject produced in recent years. One of the strengths of the book is its fine bibliography, which will serve as a good starting point for those wishing to engage with the issues he raises.

Although critical in places of views that he sees as misguided, Richter avoids the rancor that characterizes contemporary scholarship on the early medieval period of Irish history.

Richter makes clear that he is not offering a history of seventh-century Ireland but rather an investigation into aspects of the country's cultural history at the time. In particular, he seeks to explain why Ireland exported so many scholars to the rest of Europe and why in turn the country proved such a magnet for foreign scholars anxious to improve their Latin learning. To focus on these crucial and interrelated issues is a valuable exercise, particularly bearing in mind how little space they have found in the few surveys of early medieval Ireland published in the last quarter of a century.

It is a pity, therefore, that Richter's book cannot be recommended with more enthusiasm. In terms of structure and style, it leaves much to be desired, reading for the most part like a series of distinct essays put together in some haste at a late stage of production. Repetitiveness is a problem throughout. We read that "The Latin Roman empire had died gradually. In Italy the Lombard invasion of 568 was the key event in this regard" (p. 16). We are told that "The Roman Empire in the West disappeared, gradually; its end can be associated with the establishment of the Lombards in Italy in 568" (p. 17). The reasons why Colum Cille should not be considered as a *peregrinus* appear more than once (pp. 43–44 and again on pp. 49–50). Particularly in the introduction and chapter one, which purport to offer background on what is to follow, the plethora of short, unlinked paragraphs and long quotations serve merely to bewilder (see p. 23). The English is so strained in places as to suggest direct and unsatisfactory translation from another language. This seems the only explanation for the opening line of the epilogue. "The state of learning in pre-Carolingian Ireland—miracle or myth?"—this was the question which Edmondo Coccia posed a generation ago" (p. 234). We also read that "Our information about seventh-century Ireland is necessarily fragmentary, and it is a matter of opinion to what extent the information which we do have can be generalized from" (p. 20). One hopes the humor in the statement that, in the seventh century, "Ireland was important as a Mecca for scholars from abroad" is intentional (p. 137).

Nonspecialists will find this book difficult to use—some of the many Latin quotes are translated, most are not—while experts will come across little that is new. Richter provides convincing evidence (in chapter four) for the continuation of Irish influence in Northumbria into the eighth century, and no doubt his suggestion that the troublesome Easter tables used by the Irish were originally imported from Britain will raise some eyebrows, but his arguments are strongest when they draw on the work of others. The merit of this book lies in its distillation of scholarship as pioneering as that of Thomas Charles-Edwards on the *peregrinatio* and of

Aidan Breen on Irish exegesis. As such, Richter's book can be welcomed, but a satisfactory study of Ireland's relations with its neighbors in the seventh century remains to be written.

BRENDAN SMITH
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RICHARD ABELS. *Alfred the Great: War, Kingship and Culture in Anglo-Saxon England*. (The Medieval World.) New York: Longman. 1998. Pp. xviii, 373. £14.99.

There have been a number of books about King Alfred the Great of Wessex published in the run-up to the 1100th anniversary of his death in 1999, but this is by far the best of them. Richard Abels has produced a work for which there has long been a vacancy on the market: that is, a scholarly, accessible biography of the ninth-century king that provides both an introduction to the latest research and an overview of the reign. Chapters describing the main phases in the king's career are followed by others examining his military and economic reforms, his translations and campaign to revive Christian learning, and his laws and methods of government. An impressive amount of material is covered in a relatively short space with trenchant commentary provided. Some of the most original contributions concern Alfred's wars with the Vikings, where good use is made of evidence of Viking activities in Francia to help interpret the accounts in Anglo-Saxon sources. There are particularly interesting observations on tactics, with discussion of the exact location of battle sites and of the archaeological evidence for weapons and armor, and on treaty-making between Anglo-Saxons and Scandinavians, where the case is made for Alfred anticipating his descendant Æthelred II in having to buy off his enemies through the payment of tribute. Abels would support those who see the Vikings as formidable opponents, but without that meaning they have to be depicted as excessively blood-thirsty heathens looking for every opportunity to practice such delights as "the blood eagle torture."

Alfred is one of the few Anglo-Saxon rulers for whom it is possible to write such a modern biography, thanks mainly to him having had the foresight to commission his own *Vita* from his Welsh adviser, Asser. Abels joins the vast majority of scholars who have leaped to the defense of Asser's *Life of Alfred* following the claim by Alfred Smyth (in *King Alfred the Great* [1995]) that it was a forgery of the tenth century; the issues are usefully rehearsed in an appendix. Although by no means as adulatory as Asser, who had a patron to please and rewards to gain, nor historians from the late Middle Ages to the nineteenth century, who took Asser at face value, Abels nevertheless emerges at the end as impressed by Alfred and his achievements. We do not have to take Asser's word for them. Alfred's laws and translations of Latin texts suggest that he saw himself as a ruler in Old Testament

mode who had been put on earth to carry out God's will, while surviving fortifications and works of art like the Alfred Jewel testify to his innovations in these areas. Although a cynic might be suspicious of the fact that nearly all our sources for the reign seem to emanate from Alfred's court, it is extremely difficult to get at a darker side of Alfred. Only occasionally does a mask seem to slip, when we see, for instance, Alfred apparently annexing church lands in spite of his avowed intentions of promoting ecclesiastical interests, or suppressing the claims of his nephews. Abels touches on such complexities but wisely recognizes the impossibility of providing for an early medieval ruler the type of analysis to which more recent leaders might be subjected. The study also acknowledges the decline in Alfred's reputation from its high point in the latter part of the nineteenth century, when he was idolized as "the most perfect character in history." The decline was not so much due to him having been placed on an impossibly high pinnacle but to an unwarranted association with imperialism and racial Anglo-Saxonism by over-enthusiastic supporters on both sides of the Atlantic. The hostile reaction to the statue of the king erected at Alfred University in New York state in 1990, briefly discussed in the volume, suggests that some of that negative aura clings to him still. It is books like this that may eventually allow Alfred to rise above the stigma of being a Dead White European Male in the popular imagination and take his rightful place as one of the most intriguing characters in history, who apparently through his own commitment to office managed to turn around a country and a culture that appeared to be in danger of terminal decline. May not the study of how he did that still have some relevance, as well as interest, today?

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DONALD J. KAGAY and L. J. VILLALON. *The Final Argument: The Imprint of Violence on Society in Late Medieval and Early Modern Europe*. Rochester, N.Y.: Boydell. 1998. Pp. xx, 209. \$72.00.

GUY HALSALL, editor. *Violence and Society in the Early Medieval West*. Rochester, N.Y.: Boydell. 1998. Pp. 230. \$72.00.

Violence has long been a trait associated with medieval European society. The two volumes of essays here reviewed thus address a central facet of medieval social experience. The volumes share certain characteristics. Both cover broad geographical and time spans. The eleven essays edited by Guy Halsall range from the seventh through the eleventh centuries and touch on Visigothic Spain, Francia, Italy, Ireland, Viking Scandinavia, and England. Donald J. Kagay and L. J. Villalon's collection casts an even broader net. Its twelve essays encompass the seventh through the nineteenth centuries and deal not only with West-

ern Europe but also with Safavid Iran and Portuguese possessions along the West African coast. Both volumes also demonstrate the problems of trying to define violence, a term that can be stretched to cover phenomena ranging from warfare to crime to various forms of psychological intimidation. In fact, one of the more interesting of the essays under review is Halsall's introduction, in which he unpacks some of the many meanings associated with the term, discussing violence as a means of dispute settlement, of defining and maintaining social and gender relationships, as well as the ritualized behaviors that could attach themselves to certain types of violent activity. He also tries to make distinctions between "strategic" violence, designed to draw attention to but not resolve a dispute, and "tactical" violence, aimed directly at the achievement of a particular end.

In a review of this length, it is not possible to touch on all the essays in these two volumes. Instead, I will limit myself to remarks on a few of the general patterns that emerge. Not surprisingly, given the greater abundance of court records for the period after 1200, the essays in *The Final Argument* are more concerned with the actual functioning of criminal law courts and what we might call the "social location of violence": that is, who committed it and who were its victims. James R. King draws attention to the surprisingly frequent involvement of clerics in the violence of the mid-thirteenth-century Barons' War in England; Cynthia J. Neville analyzes how war with the Scots shaped the attitudes of English jurors toward female offenders along the northern frontier; Patricia R. Orr discusses why women in English courts were often allowed to make appeals of felony in cases where a strict interpretation of the law would have made that impossible. Timothy J. Coates offers a very interesting essay on how penal exile was used by the Portuguese in the fifteenth century as they expanded down the West Coast of Africa and into the Atlantic islands.

Several essays touch on the subject of feud. In the Halsall volume, Paul Fouracre discusses attitudes toward violence in seventh and eighth-century Francia, and Matthew Bennett analyzes political violence in eleventh-century Normandy. They argue that historians have often wrongly identified various forms of customary vengeance and political struggles as feuds, a position enthusiastically seconded by Halsall in his introduction. Although the question of whether "feud" is the appropriate term to apply to many of the conflicts we find in medieval Europe is not directly raised by Villalon, his demonstration that kinship ties had relatively little effect on how certain Castilian noble families aligned themselves in the political contests of the fifteenth century tends to support the argument of Halsall and his colleagues.

Another group of essays offers interesting insight into how violence could be used as a form of communication or display to underline social status and gender identity. In the Halsall volume, Ross Balzaretti shows that in Langobard Italy the law, which envisaged

free adult males as the only people who could legitimately behave violently in public situations, helped constitute men and women as gendered subjects. In the Kagay and Villalon volume, Kagay's own essay on the development of the code of the duel in Iberia and John K. Brackett's essay on violence as a "language" in the sixteenth-century Tuscan Romagna demonstrate how violence could be used as a factor in the calculus of honor that was so important to the attribution of social status in late medieval and early modern societies.

Although neither of these volumes constitutes a unified whole, they both contain intelligent, well-written essays that will repay the attention of anyone interested in the phenomenon of violence in the Middle Ages.

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JOHN FRANCE. *Western Warfare in the Age of the Crusades 1000-1300*. Ithaca: Cornell University Press. 1999. Pp. xv, 327. Cloth \$49.95, paper \$19.95.

The pace of publication is unfortunately now so quick that, if students are to be made familiar with recent trends in historical research, new works of synthesis are needed frequently. It is therefore easy to justify a new survey of Western warfare in the Middle Ages. John France's book is clearly based on a considerable amount of detailed research and reading. It adopts, in the main, a thematic approach and covers not just the obvious topics, such as the recruitment of troops, sieges, and battles in the field: among the less familiar subjects treated are metallurgical developments, the provision of food supplies, transport, and the qualities demanded of a successful commander. Medieval warfare is also set in political, social, and economic contexts, with an examination of the causes and objectives of war. Although this work can be seen primarily as a synthesis, the author is not unwilling to put forward his own views both on particular events, such as the Christian defeat at the Ager Sanguinis in 1119, and on general developments.

France is rightly reluctant to see too much uniformity in the trends of castle design or in the tactics adopted in battles in the field. He stresses the importance of land as a source of wealth and argues that "proprietary warfare" predominated in the period from 1000 to 1300, in the sense that landed possession was always a powerful influence. This is a useful and interesting point, although the argument could have been more precisely elaborated. He further draws attention to other factors that were influential in determining the conduct of war. The limited strength of governmental institutions was obviously of importance, often necessitating a resort to the use of force. France also underlines the effects of geography and climate, although more weight in this context could have been given to winter campaigning in the Baltic

region, when marshes, rivers, and sea were frozen and communications were in some ways made easier. He lastly emphasizes the significance of the state of technological development. On this point, he argues that the ad hoc nature of armies, brought into existence only when needed, affected the development of military technology. The book provides a thoughtful discussion of many aspects of warfare.

France does, however, tend to draw his examples from certain regions to the neglect of others. Although warfare in the Holy Land and some campaigns in Italy are discussed in considerable detail, references to conflict in the Iberian peninsula are few and usually rather perfunctory: even in a chapter entitled "Europe, Ideology and the Outsider," only one short paragraph is devoted to Spain. Curiously, much more space is given in this section to Anglo-Scottish relations. Some use is made of *De expugnatione Lyxbonensi* and the *Crònica* of James I of Aragon, but the overall impression is of limited reading on Spanish affairs. Since the book questions the existence of a universal obligation of military service, it would have been appropriate to consider a clause in the twelfth-century *Usatges de Barcelona* that states—without any reference to a landholding qualification—that all men who are of age and capable of fighting, both mounted and foot, should respond to the prince's summons when he needs assistance for mainly defensive operations. This clause was still being used to justify demands for service at the end of the thirteenth century.

More importantly, the book does not altogether master the problems of organization and presentation. The thematic approach is not maintained throughout the book: the last chapter is devoted to crusades and warfare in the Middle East. There is a considerable overlap in subject matter among chapters and also a very noticeable amount of repetition: for example, comments about the Statute of Winchester of 1285 already made on page 67 also appear on page 132, and the same quotation from Vegetius turns up more than once. At times, the argument is also in danger of becoming submerged in a mass of detail, and there are some inconsistencies. As the book contains much useful material and stimulating comment, it is a pity that these weaknesses were not addressed in the editorial process. General surveys that say more than the obvious are, however, notoriously difficult to write, and it is much easier to criticize than to produce one. But this book would also have been improved by the inclusion of more maps and plans and a more comprehensive index.

A. J. FOREY,
EMERITUS
University of Durham

ALEXANDER MURRAY, *Suicide in the Middle Ages*. Volume 1, *The Violent Against Themselves*. New York: Oxford University Press, 1998. Pp. xxii, 485. \$49.95.

The present volume represents the first of a projected three-volume study of suicide in the Middle Ages, focusing on the period *ca.* 1000–1500. It summarizes in serial form each genre of source, surveying the raw data concerning suicide or attempted suicide found in chronicles and legal and religious sources, going far beyond the rather impressionistic treatments of earlier scholars. Alexander Murray notes that the historian is confronted with methodological problems in identifying an act of suicide (legally classified as a felony). It is an intrinsically obscure crime, since the suicide sought secrecy, his crime entailed both loss of honor and confiscation of property, and interested parties might be loathe to report such cases. Cases of murder might be concealed as suicide. Euphemisms were employed to clothe acts of self-violence, and suicides were often portrayed as accidents. Should the victim be judged insane, he or she was not guilty of a crime.

Chronicles, for example, deal largely with the nobility, who could voluntarily choose to die in battle rather than take their own lives, and therefore exclude the treatment of other social classes (although German urban chronicles, due to their interest in the day-to-day workings of the law and their desire to note its sanctions, often contain detailed reports). Furthermore, the spotty and incomplete nature of judicial records and the often splintered and overlapping jurisdictions into which Europe was divided will not allow a full and accurate accounting of the frequency and specific circumstances of such cases. The more centralized royal authority in England and the institution of the coroner produced the earliest recorded judicial cases (late twelfth century), and English cases outnumber all others. In France, letters of remission provide some of the most detailed reports, although these may be somewhat skewed in order to save the survivor from punishment. Murray therefore begins each survey with a summary of the particular difficulties that impede an understanding of these sources, continuing with very detailed summaries of the cases cited in each genre. As a result, while each case provides some interesting insights into social history, this volume nevertheless often reads very much like a raw appendix of sources that might be better placed following the forthcoming volumes.

The cases found in hagiographical and canonization records were among the most widely known, reaching the public through *exempla* (e.g. the work of Caesarius of Heisterbach and Gerard of Fracheto) to be inserted into sermons or by means of visual images. These cases, however, largely deal with attempted suicides saved through supernatural intervention and contain an implied or stated moral message. The miracle collections of Saints James of Compostella, Mary of Rocamadour, Foy, Nicholas of Tolentino, and Margaret of Cortona, for example, provide us with rich material, which is examined serially by the author.

In the course of the chapters dealing with each literary genre and a summary chapter, which provides statistical coverage, Murray attempts to draw a picture

of the medieval suicide. Men clearly dominated by at least two to one, usually three or four to one. Among the stated motives were distress over debt or an economic loss; unrequited love; remorse over theft, incest, abortion, infanticide or murder; postnatal depression; the loss of a spouse, lover, or child; the unbearable pain of torture or imprisonment; and melancholy and madness. The preferred method was solitary hanging, preferably at home or in a prison cell. Florentines seem to have had a particular tendency to suicide (according to Dante); the poor were often unreported, because they lacked property, although most suicides were probably rural folk. Women preferred April, men July; Mondays and dawn or prime dominated. Until the sixteenth century, suicide rates appear to have been considerably lower than during the modern period, although the paucity of sources may explain this; Murray argues that the centralized state and the emotional trauma of religious change during the Reformation may be responsible for the increase. An attempt is made to compare suicide and homicide rates, but it would seem that the evidence is not complete enough to draw firm conclusions.

Much of the book contains tantalizing hypotheses and suggestions, but the reader is frustratingly referred to the later volumes, which apparently will be more analytical and will survey the medical, theological, and philosophical literature and the affective aspects of suicide during this period. Martyrdom—although not by the victim's own hand, surely to be classified as a form of suicide—plays no role in this study, despite being one of the defining values encouraged in medieval culture among both the orthodox and some heretics (e.g. the Cathar *endura*). Reference to Muslim and Jewish traditions might be illuminating. For example, Israel J. Yuval's article, "Vengeance and Damnation, Blood, and Defamation: From Jewish Martyrdom to Blood Libel Accusations" (*Zion* 58 [1993]: 33–90), concerning the cultural ramifications of the alleged mass suicide and child murder of Jews that accompanied the anti-Semitic outbreaks of the First Crusade, has stirred heated public and scholarly debate.

In sum, Murray provides an admirable summary of the raw material of actual suicides in the Middle Ages drawn from a rich variety of sources, whose effective mining often requires considerable spadework. His graphic prose displays a sensitivity not merely to the dry data but also to the human tragedies these tales reveal. The forthcoming volumes, which will flesh out our understanding of how suicide was understood by contemporaries and what this tells us about medieval culture, will be eagerly awaited.

MICHAEL GOODICH
University of Haifa

JEAN-CLAUDE SCHMITT. *Ghosts in the Middle Ages: The Living and the Dead in Medieval Society*. Translated by TERESA LAVENDER FAGAN. Chicago: University of Chicago Press. 1998. Pp. xiii, 290. \$33.00.

It was the sad fate of ghost stories in the Middle Ages that their terrors inspired rather than intimidated the church. The specific character of these tales—alternately horrifying and pathetic—was lost as clerks told, retold, and eventually reduced a diverse corpus to a handful of illustrative morals. In Jean-Claude Schmitt's book, ghost stories undergo something of a reversal of fortune: at points, the author's celebration of the number and variety of these narratives overwhelms the conclusions that his survey was designed to produce.

The book is divided into nine chapters, each with many subdivisions. The first chapter supplies a quick but informative trip through patristic teachings, all of which prepare for the breakthroughs that conveniently emerge around 1000 (these are given the briefest possible attention). The final two chapters, both excellent, explain the relation of ghosts to time and social space and analyze images of the dead. The intervening chapters, packed with summaries of ghost stories and arranged by genre, become less compelling as they become more like catalogues.

Schmitt's book is nonetheless a fascinating inquiry into the impact of the dead on the living, although the book's major point is, mercifully, that the dead do not have the upper hand. "The dead have no existence other than that which the living imagine," Schmitt writes. But more than imagination—that is, the "imaginary," the faculty with the wit to shape images—is involved in this tale of growing ecclesiastical control over the symbolic networks in which ghost stories were enmeshed (p. 67). The dead came back to life for many reasons, most of them, it turns out, having to do with the church's insatiable appetite for the laity's funds: to haunt those who did not bury the dead properly or who failed to pray for them or otherwise heed the rituals that accommodated the living to loss. *Memoria*, "the remembrance of the dead," served more than monetary interests, of course; it assured that eventually pain would diminish as the recent dead joined the long dead in commemorative liturgies and rituals. *Memoria* thus functioned ironically as "a social technology of forgetting" (p. 5).

From about the year 1000 on—this being the date from which Schmitt claims substantial evidence—the church took firm charge not only of that technology but of most means through which the dead were allowed to reach the living. Once Purgatory was established, ghosts reassured the living that prayers, alms, and Masses would secure happiness for the dead. Schmitt seldom misses an opportunity to portray the church as a masterful manipulator of the laity's experiences of death. He points out that the lamentable "banalization of tales of apparitions of the dead" (p. 136) itself played a part in the church's plan to make the laity more familiar with death through ritualization of funerals and the sacralization of cemetery space. If visits from the dead were made to seem less extraordinary, that was because death was intended to be seen as an ordinary part of ecclesiastical time and place.

Too much of the book is taken up with individual narratives, but Schmitt has a fine sensibility for making discriminations among the various genres in which medieval ghosts appeared. Especially good is the discussion of illuminated manuscripts portraying ghosts (the book includes a rich gallery of thirty illustrations, most of them in color). On the debit side is the author's failure to treat earlier evidence. The Old English *Soul and Body* poem (which exists in two forms, a great rarity in the corpus of Old English verse) and related prose and verse might have been worth considering here. In these works and in earlier Celtic texts, a soul revisits its body at regular intervals. *Soul and Body* describes the soul's ritual return to chastise the body for dissolute living, the result of which is the soul's torment. In one version only, the damned soul reviles its body, but in a second version both saved and damned souls return. Were these ghostly visits not worth including? They are, after all, vernacular narratives, and Schmitt notes that evidence in the vernacular is sparse (pp. 80, 201).

It is striking that Schmitt limits his inquiry to texts in Latin when important evidence also exists in vernacular languages. At one point, celebrating the linguistic diversity of the Anglo-Norman realms, he asks, "Is it not the diversity of cultures that hones the minds of men and women?" (p. 80). Language is part of that diversity, and Schmitt's book suffers from the self-imposed limitations that he has brought to his subject matter by his focus on Latin texts. A second self-imposed limitation is his decision to ignore evidence from before 1000. *Soul and Body* and its kin have their origins in the early Middle Ages, which had a Latin and vernacular discourse of nightmares and ghostly visitations that might not necessarily confirm Schmitt's conclusions. This is an engaging, gracefully translated, and well-produced book. But it is a shame that "The Middle Ages" should be defined so narrowly in a book that promises so much.

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KATHLEEN CUSHING, *Papacy and Law in the Gregorian Revolution: The Canonistic Work of Anselm of Lucca*. (Oxford Historical Monographs.) New York: Oxford University Press. 1998. Pp. xi, 246. \$69.00.

This monograph on Anselm of Lucca's compendium of ecclesiastical law, the *Collectio canonum* (ca. 1083), attempts to set the work in its broader historical context, understanding it as both reflective of the ideology of the eleventh-century "Gregorian" reformers and a means to realize their vision of Christian society. Historians have long acknowledged the importance of law in this reform movement, and Kathleen Cushing's work contributes a detailed example of how a leading figure in the movement used law.

The figure Cushing has chosen for her analysis is an important one: Anselm was the nephew of Pope Alexander II, who preceded him as bishop of the

Tuscan city of Lucca and was closely connected to Pope Gregory VII and his circle. He was also the spiritual advisor of Countess Matilda of Tuscany, a woman who was a towering figure in the political life of medieval Italy and a key supporter of papal reform initiatives. I can not wholly agree with the author that Anselm's importance has been "underestimated" (p. 1). In his magisterial *La Réforme grégorienne* (1924–1937), Augustin Fliche noted the significance of Anselm's *Collectio canonum*; Edith Pásztor published several studies of his *vita* in the 1960s; and in the 1980s, Anselm was the subject of two major conferences in Italy. Cushing does, however, provide a judicious survey of this literature and the state of the sources.

But here is where problems begin. Cushing compellingly describes the inadequacies of the one published edition of the *Collectio* (ed. F. Thaner, 1906–1915) and the many unresolved problems in the manuscript tradition that inhibit the publication of a new, critical edition at this time. Her study is based on a "working edition" that focuses on four of the most complete manuscripts but utilizes material from eleven other recensions. An appendix contains "a provisional and much abridged working edition of Books XII and XIII," the two books not published at all in Thaner's edition. Cushing recognizes that the provisional character of the edition upon which she bases her analysis is problematic. But she has chosen to publish her partial, abridged working edition as an aid to scholars and hopes that by asking interesting questions of this source, at least "a step in the right direction may have been taken" (p. 8).

Several things need to be said at this point. First, Cushing has accomplished difficult manuscript work for a young historian. There is evidence of real erudition in this book: her appendixes and annotation reveal a careful and critical engagement with numerous manuscript sources. As a historian, I have no problem with Cushing's decision to try to push forward discussion of this figure and his canonical work by publishing a searching analysis rather than a definitive study. Scholars before her have certainly used the same inadequate texts to build arguments and have not been as forthright and conscientious in acknowledging the problematic character of the sources as Cushing is here. The author's candor in evaluating the state of the sources and scholarship is a valuable contribution. Not being a canon lawyer myself, I will leave the appraisal of Cushing's provisional edition to experts in this field. Instead I will focus on the quality of Cushing's historical interpretation of Anselm and his canonistic work.

Cushing concludes that "Anselm's collection stands as a concrete manifestation of the ways in which the reformers envisaged the law as an aim, a means, and a justification of reform" (p. 143). She contends that Anselm did not just collect canons but that he interpreted them, and that the resultant text had a reform "agenda." This agenda generally underscored papal primacy, a strongly hierarchical ecclesiology, and a more militant and coercive notion of ecclesiastical

authority. Throughout the work, Cushing evinces a strong commitment to a notion of intentionality: she reads the *Collectio* as the articulation of Anselm's decided views and sees law as a means used by reformers to achieve revolutionary goals. This interpretive stance grows at least partly out of the author's acceptance of a conception of the Gregorian reform as "the first European revolution" (p. 15).

Unfortunately, Cushing's evidence is not so decisive, and her own conclusions are anything but revolutionary. Few scholars in this postcritical theory era would argue with the notion that a text has a perspective, or agenda, but the evidence of Anselm's *Collectio* does not support the kind of single-minded intentionality that Cushing's "Gregorian Revolution" framework requires. The author's discussion of Anselm's use of the *Collection in Seventy-four Titles* (74T), particularly as a source for Pseudo-Isidorian canons, is a good example of a problem that recurs through the work. She begins with the assertion of a "wide divergence in opinion and intention between Anselm and the compiler of the 74T" and proceeds to consider "the ends towards which Anselm used texts from the 74T" (p. 80). But she almost immediately admits that, in his use of Pseudo-Isidorian texts from his source, "Anselm often remained faithful to the Ps.Isidore's intentions" (p. 81) and that "Anselm did remain almost entirely faithful to his exemplar [the 74T]" (p. 82). In some cases, however, Cushing continues, Anselm took his Pseudo-Isidorian materials from other sources that gave "a more exacting interpretation of the text in question." She gives one strong example here of Anselm's incorporation of a canon with a much more expansive and general view of papal power than the version in the 74T, but then admits that "[o]ther cases, however, where an independent Ps.Isidore was used as opposed to one in the 74T, offer less conclusive justifications" (p. 82). In the end, we have one strong example and a lot of back-pedaling from the initial premise. Indeed, the evidence offered cannot support the strong claims made at the outset.

Although the evidence of the *Collectio* is too mixed to sustain an interpretation of Anselm as "Gregorian revolutionary," the diversity within the compilation and the vexed nature of the manuscript tradition itself offer the material for a generative rethinking of the model of historical change the author has espoused. Cushing's orientation is highly traditional: she tries to discern the authorial agenda in the compilation, and she relates the views discerned to those of key reformers. This approach to the changes in the church in the eleventh century, of course, privileges the "reformers"—a group of ecclesiastics allied with Gregory VII—as the key historical actors and concentrates on elucidating their ideas and actions. In order to maintain the narrative of a group of reform-minded clerics revolutionizing the relationship between church and state, diversity in the views and actions of leading ecclesiastics in the eleventh century has been consistently downplayed. Indeed, Cushing presents evidence

(e.g. pp. 108–10) that the *Collectio* contains canons at variance with important "Gregorian" positions. More importantly, her research reveals that Anselm's compilation was not treated by contemporaries as a program of reform for implementation. Rather, "interpolations and changes to the internal ordering, as well as other modifications were made almost immediately" (p. 142). This is why the manuscript tradition is so vexed. But rather than seeing this complexity as an obstacle to understanding the "great men" of the Gregorian Revolution, why not let it help us reconsider the relationship among law, legal texts, society, and belief in a crucial period of transformation in the European past? In sum, Cushing's adherence to a highly traditional conception of ecclesiastical change has resulted in a disappointing book, one that misses opportunities to break new ground and too often offers insufficient evidence for its characterizations and interpretations.

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M. T. CLANCHY. *Abelard: A Medieval Life*. Malden, Mass.: Blackwell. 1997. Pp. xiii, 416. \$59.95.

This is one of the half-dozen best books ever written about the European Middle Ages. M. T. Clanchy integrates ecclesiastical, cultural, and social history, and the biography of a great mind and personality—that of the twelfth-century philosopher and academician, Peter Abelard—with depth of learning, subtlety of insight, and skillful and at times elegant writing that are rarely seen among medievalists. The world of the merging University at Paris in the 1120s and 1130s and the multifaceted roles that Abelard played in this context are brought to life in magisterial fashion.

There were novel insights that I brought away from this fascinating book. Abelard's autobiography would lead one to assume he was an outlander; in fact, his family was French or at least partly French. The University on the Seine had taken much fuller shape by 1130 than I realized. Along with instruction and learning came the curse of academic politics, and in that area the power and influence of the chancellor of the cathedral were crucial in shaping and advancing the careers of the teaching cohort. The chancellor in turn was much involved in royal courtly politics, and it was this secular role that gave him so much sway over the faculty. There was very wide latitude then in what a professor could say in his lectures, even on anxious theological issues. The problem for an avant-garde thinker like Abelard came when he wrote down his teachings and published them in manuscript form; then his enemies and jealous colleagues had a text they could pick away at, and they denounced him to the authorities. As in the Medieval Academy of America today, publication of a bold and interesting book, rather than the usual somnolent trash, meant publish and perish.

In this book, Heloise comes through much more

strongly as an intellectual and a vigorous personality than in any previous historical account of the renowned love affair, marriage, and sad separation. Clanchy has no doubt that the letters attributed to her in the collected manuscript correspondence of the couple were written by Heloise, and from this and other evidence it is apparent that she was Abelard's intellectual equal and played a major role in development of his thought, as she probably was the bolder of the two at the start of their affair. The son produced by this affair, who was raised by Abelard's family, ended up in a good church job in Nantes.

As portrayed by Clanchy, Abelard was not so much an unstable personality as he was an inner-directed intellectual and academic, driven by curiosity over a wide spectrum of academic subjects and with a naïve conviction that learning and reason could not but be rewarded in university and church precincts. He pressed on continuously, arousing both jealousy and fear—a familiar story at Harvard, Yale, and Princeton Universities in recent decades. The churchmen of the twelfth century had no capacity to understand an intellectual controversy in psychological terms, so Abelard fell afoul of abstract theory, of ideology, a weakness that the Roman Catholic Church of the Middle Ages was to exhibit again and again (and that has, under the current reactionary pope, not yet been superseded).

In sum, as portrayed by Clanchy, Abelard's story is this: only ten percent of academics know the truth, but they lie about it for career and ideological reasons. The other ninety percent are too dumb to know the truth. Someone who knows the truth and writes it down is victimized. Academia is no place for a first-rate mind.

Until I read this book, it had not occurred to me how close was Abelard's discourse to that of the Jewish rabbis of late antiquity: the application of reason and Greek philosophy to the making of a culture grounded in Scripture. This all seems bootless now, but it was still a thriving enterprise in the nineteenth century and in a sense continues today in academia—only grounding in liberal and egalitarian dogma, another set of irrational precepts, has replaced Scripture.

My only regret about this book is that I came to the end of it too quickly. I would gladly have remained immersed in Abelard and Heloise and their world for another two or three hundred pages. When is the last time I felt that way about a book written by an academic medievalist? Only in first reading R. W. Southern, Johan Huizinga, and David Knowles many decades ago did I have such pleasure. Clanchy's Abelard will stay with you and haunt your sensibility and imagination.

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BERNARD F. REILLY. *The Kingdom of León-Castilla under King Alfonso VII 1126–1157*. (The Middle Ages

Series.) Philadelphia: University of Pennsylvania Press. 1998. Pp. xv, 431. \$65.00.

Bernard F. Reilly has now completed the third of his studies on the Leonese-Castilian monarchy during the eleventh and twelfth centuries. Following *The Kingdom of León-Castilla under Queen Urraca, 1109–1126* (1982) and *The Kingdom of León-Castilla under King Alfonso VI, 1065–1109* (1988), we now have a study of the kingdom as it stood during the rule of Alfonso VII from 1126 to 1157. The only prior twentieth-century account of this king, Manuel Recuero Astray's *Alfonso VII, Emperador* (1979), fails to offer coverage of the entire reign and relies almost totally on contemporary chronicle evidence, offering little documentary sweep or detail. Simon Barton's *Aristocracy in Twelfth-Century León and Castile* (1997) detailed the aristocratic background. In the present work, Reilly has reverted to the topical format of his earlier Urracan study, departing from the more integrated narrative style employed in his account of Alfonso VI. One might surmise that the greater abundance of surviving documents from the years of Alfonso VII invited the categorization of the richer resources into topical frames displayed here. Reilly suggests, however, that he has rethought his approach to Alfonso VI as the framer of a national monarchy and prefers the concept of the king as a possessor of royal patrimonies rather than the creator of a precocious monarchical state. That view has controlled his approach to this book. Although the first four chapters do take the form of narration, the latter six resort to a topical investigation of the king's areas of relations, dealing in turn with his aristocratic court and council, the counts of the realm, his castellans, officials of local government, the military organization of the kingdom, tax structure, the church, and finally the towns of Leon-Castile.

Reilly began his research on these three monarchs intending to write an account of Queen Urraca's chancery, and close analysis of the royal documentation has characterized, even dominated, the nature of the subsequent works. The study includes a valuable annotated guide to the 975 documents he has utilized for Alfonso VII's period, and he has concentrated much endeavor to weigh the authenticity of each. Since many are land transactions, these documents have shaped the proprietorial image of the monarchy presented in the text. Reilly has employed the documents in a creative manner, constructing an itinerary for the king that directs the thrust of his narrative. Using diplomatic and prosopographical techniques, he has extracted valuable information regarding the various subjects addressed to extend our knowledge of the period and its personalities. Compared to Alfonso's contemporaries in England (the later reign of Henry I, Stephen's reign, and the early reign of Henry II) and Louis VII in France, royal institutional development appears rather similar but slow, with the assorted counts, castellans, and *merinos* better defined by their personalities and wealth rather than any sense of a

systematic structure into which they can be fitted. The taxation and military structures remained at a rudimentary level, given the substantial size of Leon-Castile (equal to England, Wales, and Scotland combined). These were sufficient to permit the king to wage war successfully in terms of pressing southward, but insufficient to prevent the creation of an independent Portugal or to secure the retention of Zaragoza to slow the growth of neighboring Aragon. In time, Alfonso would be overmatched in the south by the resources of the Almohads from North Africa. The king managed to avoid serious conflict with Rome while retaining substantial powers over his domestic episcopacy, and the peninsular church witnessed the steady progress of European reforms of its practices. Subtle but important developments marked the evolution of the municipalities, spurred by the resettlement of the population to the south. The growth of the town councils (*concejos*) and their right to appoint municipal officials spread in his reign, even to small places. Violence and revolution were apparently not needed to obtain this modest gain in their privileges and limited independence, and it presaged major breakthroughs in the decades to come.

Reilly perceives Alfonso VII as a "traditionalist" monarch, utilizing his titles of king of Leon and Castile and emperor of Spain largely to enhance his respect among competing forces in the peninsula, rather than embracing any kind of forward-looking policies of the growth of royal institutions or peninsular unification. These views are not new, but with Reilly's meticulous examination of the sources, we can understand better than ever before the base of the king's actions. Our knowledge of this formative era in Spanish history has been greatly enhanced through his trilogy of monarchical studies.

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FIONA J. WATSON. *Under the Hammer: Edward I and Scotland, 1286–1307*. East Lothian Scotland: Tuckwell Press. 1998. Pp. xxvii, 255. £14.99.

A bleak period for medieval Scotland began on the morning of March 19, 1286, when King Alexander III was found dead on the seashore in Fife. His granddaughter Margaret succeeded him in the monarchy, but the real power was in the hands of a council known as the Guardians. Very quickly they began to involve Edward I of England, the great-uncle of Margaret, in Scottish affairs. After Margaret died in September 1290, the Scots turned to Edward to judge the claims of those who wished to be king. Edward used the opportunity to have his overlordship recognized (a claim he had made as early as 1278), which was the spark that would ignite the conflagration that was the Anglo-Scottish wars of the early fourteenth century.

This book, with its title's witty reference to Edward's fame as the *malleus Scottorum* ("hammer of the Scots"), looks at one aspect of those wars: the English

administration of occupied Scotland. The tasks of raising troops, leading them to the proper place, setting up garrisons and then supplying them were as important to Edward's goals as battles and skirmishes. The majority of the book looks at the period 1296–1305: from the deposition of John Balliol, when Edward took (or attempted to take) direct administrative control of Scotland, to what would prove to be the temporary Edwardian triumph.

In this important study, Fiona J. Watson has collected and analyzed material on the economics of warfare and occupation: the cost of the occupying force, the amounts of foodstuffs necessary to maintain that force, and the monies that Edward was able to raise from the Scots in taxes. The narrative of the book moves between a description of political and administrative events to a close study of the economic data. Edward's obsession with control of Scotland is revealed by the tremendous outlay of capital needed for his campaigns. Occupying Scotland was not financially profitable, despite the persistent orders from Edward to his officials to make it so. Moreover, the English campaigns into Scotland were a crippling expense for the northern English counties that were forced to bear much of the burden.

Surprisingly for an examination of the English administration, this study is narrowly Scottish in sympathy. Edward is "persistently belligerent and overbearing," while there was at Stirling an "evil English garrison." In contrast, the Scots apparently could do no wrong. Throughout the book, there is little comment on the historiographical problems with the records used by the author. This is particularly important for the historical accounts from contemporary writers, notably Walter of Guisborough and Walter Rishanger, who describe the battlefield conditions at second hand and popular responses to Edward's policies at first hand. The two Walters were outspoken supporters of the English baronage against the monarchy, and their criticisms of Edward have to be considered in that light. This is particularly true for Walter of Guisborough, whose credibility must be set against his tendencies toward the dramatic. An additional reason to proceed warily when using his Scottish information is that his monastery had been founded, and patronized, by the Bruce family, whose relations with Edward were convoluted to say the least. Walter Rishanger, writing at St. Albans, has members of the English army dying from starvation in the midst of prime Scottish arable land at the beginning of the harvest. Finally, the discussion of the administration of the occupied lands and the occupying force would have benefited from a greater understanding of how governments and armies operated in practice during the fourteenth century. Desertions from garrisons, discrepancies between numbers of men summoned and those who actually served, or the amount of foodstuffs ordered and the amounts actually received are not necessarily signs of English incompetence or fatigue. For most of the Middle Ages (and well into the

modern era), these are normal problems that any government or army had to face.

This book looks at a subject that will probably not find its way into popular culture, but an understanding of it is essential for scholars. Such a study has long been needed, because the fabric of military occupation in Scotland helps to explain many obscurities of the time. Watson has written a work that will be valuable to students of British history as well as those interested in medieval and Scottish administration.

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JOSEPH ZIEGLER, *Medicine and Religion c. 1300: The Case of Arnau de Vilanova*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University 1998. Pp. vii, 342. \$80.00.

In this study, Joseph Ziegler reexamines the relationship between religion and medicine during the high and later Middle Ages. The reception in the Latin West of a substantial body of technical medical literature from Greek and Arabic sources and the rise of the universities of Bologna, Paris, and Montpellier as centers of academic medical learning unquestionably raised the profile and enhanced the status of medicine among the arts and sciences. Consequently, much of the considerable body of recent scholarship on the history of twelfth to fourteenth-century medicine focuses on access to new sources of secular knowledge and the development of medicine as an academic discipline that was secular in content (even though in some instances practiced by clerics). Ziegler's revisionist interpretation instead emphasizes the many ties between religious and medical personnel, institutions, and ideas during this period. He further argues that in late medieval Christian society, religion and medicine must surely have permeated one another at many levels.

For the latter contention, his paradigmatic case is that of Arnau of Villanova. Both Arnau's career and his writings have often been treated as falling into two separate and unrelated categories, each occupying a distinct phase of his life. According to this view, his numerous medical writings were the fruit of his service as royal physician and professor at Montpellier, whereas his spiritual and eschatological works belonged to his completely separate role as religious reformer and prophet. Instead, Ziegler finds "fusion rather than disjunction between Arnau's medical and spiritual writings" (p. 268), basing his conclusion primarily on linguistic analysis of Arnau's works. Like all medical writers, he used technical terms that had both specialized medical and more general moral or religious meaning (for example, *malitia*, *corruptio*). Perhaps more significantly, Ziegler shows that in Arnau's spiritual writings and endeavors to defend and justify his religious ideas, he expressed the notion that the *medicus fidelis* may in some sense be divinely inspired. Nevertheless, his medical works contain very little in

the way of religious ideas or specifically religious expressions, while any medical ideas or vocabulary in his spiritual works appear to be very general and abstract.

Despite Ziegler's careful contextualization of his views, Arnau remains an anomalous case. Few (if any) other learned physicians played as major or as challenging a religious role as he did. But notwithstanding its title, this book does not concern Arnau alone. Ziegler has also identified another "theologizing physician" among Arnau's contemporaries, the Genoese Galvano da Levanto. This author deliberately paired a medical treatise on joint diseases with a spiritual work on the health of the soul. In the latter, moreover, he freely employed medical terms considerably more specialized than those in Arnau's religious writings in order to moralize about the body and deploy physiological metaphors about the soul. In addition, Ziegler has carried out a detailed analysis of the language of several early fourteenth-century clerical authors without known medical background who made extensive use of medical terminology in religious writings. The result is a fascinating and extremely rich exploration of the broad dissemination of specialized medical language in high medieval society and culture. Thus, the Dominican Giovanni di San Gimignano, author of a *Summa de exemplis et rerum similitudinibus* for preachers, drew on a vast store of metaphors about the body, its organs, functions, health and disease—in effect a moralized physiology. To be sure, some of these metaphors were traditional. Yet as Ziegler points out, the frequency with which they occur, the high level of specialized terminology employed, and the citation of medical authorities by name (often at second hand from encyclopedic authors) all suggest a wide diffusion of medical knowledge. Indeed, these phenomena imply the existence of lay audiences familiar with the subject and language of medicine and receptive to learning more of it from preachers as well as physicians or encyclopedists. A concluding chapter provides a thoughtful evaluation of the occasional areas of tension and much larger measure of cooperation between priest and physician.

The general conclusion that thirteenth and fourteenth-century Christian medical practitioners by and large participated fully in the dominant religious culture is surely correct, as well as consonant with much of the most recent work about various medieval learned disciplines. Nevertheless, as this interesting study itself indicates, it is much easier to find medical metaphors or vocabulary in medieval religious writings than religious language in medical treatises, most of which are to an overwhelming extent purely technical in content.

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EUROPE: EARLY MODERN AND MODERN

ROBERT B. EDGERTON. *Death or Glory: The Legacy of the Crimean War*. Boulder, Colo.: Westview. 1999. Pp. ix, 288. \$30.00.

This is a book of striking contrasts. It is based on an impressive assembly of the extant literature in many languages, and the dustjacket promises a narrative immersion into the conditions of the Crimean War, rather than "a mere battle narrative." To support this contention, Robert B. Edgerton, who has written widely on military culture, brings to his work a range of insights and methods that complement more prosaic talents. He asks whether the national culture of the combatants made any difference to the way they fought and makes comparisons with the the American Civil War.

Unfortunately, the results do not live up to the advance billing. The opening chapter—on the origins of the war, events preceding the invasion of the Crimea, and the wider conflict—is littered with simple errors of fact. These errors, which a closer reading of "battle narratives" could have avoided, call into question the integrity of the book. The next chapter assesses the armies that fought in the Crimea and can only be described as a parody of modern scholarship. It repeats all the old canards about military incompetence and add a few novelties. The image of the British commander, Lord Raglan, remains a caricature, ignoring a recent biography and a study of his spy network, as well as indications in Edgerton's text that he was a professional and effective officer. At the Alma and at Inkerman, Raglan's intervention was decisive, while his ability to maintain the confidence of a succession of French co-commanders was essential to the coalition. There was nothing strange, as Edgerton alleges, in the hard-boiled General Pelissier weeping at his deathbed; Raglan inspired the devotion of all who worked with him.

The book is stronger on the wider aspects of the campaign, medical and logistics problems, the largely unrecognized role of women and children in the combat zone, and the contribution of the Turkish armies. The highlight is an effective analysis of the Russian siege of Kars, which was resolutely defended by Turkish troops under British command. I was, however, unable to distinguish the section from "a mere battle narrative." Two final chapters address the human response to combat and the wider horrors of war. They reveal a steady loss of innocence; a degradation of the prewar ethos, neatness, and discipline that professional armies prided themselves upon; a process that contrasted with the growth of fatalism, survival instincts, despair, and at least one case of "shell shock." These are the most effective sections of the book.

The major problem of this book is the uncritical handling of a complex, nuanced literature. So much of what was written in Russia, France, and Britain addressed contemporary agendas. The French emperor

needed a propaganda triumph and commissioned appropriate pamphlets. In Britain, the Administrative Reform Association waged a political campaign to replace aristocratic government with a meritocracy. Much of the war reporting reflected their agenda, demonstrating the incompetence of simple-minded, amateur aristocratic officers. So much of the contemporary literature cited by Edgerton, from the beatification of Florence Nightingale by *The Times*, to meet the needs of its middle class audience, to the legendary massacre of the Light Brigade by three bungling aristocrats, Lords Raglan, Lucan, and Cardigan, was unreliable. By contrast, A. W. Kinglake's *The Invasion of the Crimea*, published in seven massive volumes between 1863 and 1887, was written as a modern *Illiad*, with poor Lord Raglan once again woefully miscast, this time as a modern Achilles.

Edgerton also adopts the "unnecessary war" thesis advanced by diplomatic historians more concerned to teach their own generation the folly of war than to address the underlying rivalries of the age. The Crimean War was neither unnecessary nor barren of results. Britain, France, and Russia fought on a global scale for mastery of Europe—a prize that went, temporarily, to the French—and mastery of the world, which the British retained for another generation. The war removed Russia from Central Europe for ninety years, facilitated the unification of Germany, and changed the strategic geography of the Baltic region.

Taking a thematic cross section of combat does not provide "immersion" in the culture of soldiers, and the judgment that the grim business of war reduces soldiers to a common level is hardly novel. Building this immersion on uncritical use of an old literature of the war casts doubt on the validity of Edgerton's work. This is unfortunate, for the book is commendably ambitious.

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AVIEL ROSHWALD and RICHARD STITES, editors. *European Culture in the Great War: The Arts, Entertainment, and Propaganda, 1914–1918*. (Studies in the Social and Cultural History of Modern Warfare, number 6.) New York: Cambridge University Press. 1999. Pp. xii, 430. \$64.95.

Borderlands are by definition realms where certainty wilts and categories fade. In borderlands there are no right answers. Ambiguity is the norm. Perhaps the most spectacular borderlands of modern history have been those of Eastern Europe, where frontiers and power have shifted endlessly since the great tribal movements of the fourth and fifth centuries A.D. The changes have been so frequent and confusing that Eastern Europe remained until not long ago the domain of the super-specialist, a researcher driven often by ethnic allegiance and hence suspect to the guild as a whole. The Eastern Europeanist was bound

to be a partisan, it was thought, often of fanatical stripe, and hence deservedly marginalized.

How things have changed! Today Eastern Europe is in. The history shelves of our megabookstores overflow with volumes on Budapest, Prague, Bosnia, and Ukraine. Norman Davies, in his invigorating survey of Europe, has even argued that Poland is the epicenter of the European experience as a whole. Whence this new focus and interest? Aside from obvious explanations pertaining to the end of the Cold War and new opportunities for travel and research, the very diversity of the Eastern European past—its multiple meanings—seems somehow to speak engagingly to us. The Eastern European experience, with its motifs of turmoil, imprecision, and confusion, may somehow prefigure the postmodern conundrum.

If so, then a volume on the cultural impact of World War I on Eastern Europe is surely welcome, not only because this side of that Great War—the Eastern Front—has been decidedly neglected in the historiography but because such an investigation could offer important insights on the broader question of the loss of categorical certainty and the emergence of modernism and postmodernism. What happens to the imagination in borderlands when central authority comes under attack and then crumbles, as it did so sensationally in Eastern Europe during and after the Great War, when the Romanov, Habsburg, Hohenzollern, and Ottoman empires collapsed?

I wish I could say that the volume of fourteen essays that Avieli Roshwald and Richard Stites have put together is an indispensable read. The intent was laudable; the results are mixed. The emphasis is indeed on Eastern Europe—on Poland (Harold Segel), Austria (Steven Beller), Hungary (Joseph Held), Romania (Maria Bucur), Bulgaria (Evelina Kelbetcheva), and the lands of the Czechs (Claire Nolte) and the South Slavs (Andrew Wachtel)—but not exclusively. Stites has, as always, a most readable contribution on Russia, and Roshwald reflects on Jewish identity across a variety of frontiers. Then a series of essays reverts to more familiar ground: Peter Jelavich, Marc Ferro, Jay Winter, and Walter Adamson contribute their eminent thoughts on Germany, France, Britain, and Italy, respectively. Finally, Sophie de Schaepdrijver deals with Belgium, in what may be the most evocative piece in the volume. She shows how Belgium, united at the outset of the war, threatened to come apart by its end, divided irreparably by the bitter struggles between Flemings and Walloons. “Brave little Belgium,” the center of the world’s attention in August 1914, had become, four years later, a hollow trope, a borderland of the Allied soul. The wartime German notion of Belgium’s insignificance won out over the Allied idea of heroic Belgium, and Geneva, not Brussels, became the site of the League of Nations.

The Eastern European and Russian chapters have a similar inherent irony, but rarely is this developed appropriately. Divided loyalty was commonplace among Poles and Czechs, not to mention Baltic Ger-

mans. How could increasingly self-conscious “subject nationalities,” or the socially aggrieved of Eastern Europe, fight on the side of imperial authority? What did such conflict do to imaginative endeavor, to art, literature, music, and poetry? In the short term, the difficulties were of course papered over. Individual interest was made subservient to the cause. The Symbolist poet Zinaida Gippius, for instance, wrote in December 1914 that “it is a sin to write poems now.” But that attitude did nothing to eliminate the conflict in her heart. Publicly she appeared to support the war; privately she opposed it. She composed the wonderfully ambiguous story of the little Latvian boy whose Russian schoolmates called him “the German” on account of his strange name. When his mother informed him that he was a Latvian and not a German, he was relieved: as a Latvian he could, he was told, also consider himself a Russian!

Such ambiguity may have been repressed in the borderlands during the war itself, but it was essential to the situation and was to surface with a vengeance in the aftermath to produce political instability and civil war, often accompanied by elemental cruelty. Even then, the ironies and contradictions were not necessarily articulated. Dada never did seem welcome east of Berlin, and Jaroslav Hašek, creator of “good soldier Švejk,” had no prominent emulators elsewhere. No one, for example, came along to document the bizarre developments in the Baltic region, where in one “theater of war” (the term was never more apt) in late 1919, a British soldier, Colonel Alexander, who only months earlier had fought against the Germans on the Western Front, now commanded a contingent of German and Russian troops against the Bolsheviks. When Kiev, capital of Ukraine, changed hands sixteen times between the end of 1918 and August 1920, the assault on logic, continuity, and history was similar. Just because a Tom Stoppard did not surface to enunciate the perplexities does not mean that they were not there or that they disappeared without trace.

The main problem with this volume of essays is that it does not pursue these ambiguities and contradictions into the postwar years. Contributors were apparently instructed to stick to the war years, to document cultural expression between 1914 and 1918, and not to venture into the realm of memory and the postwar rituals of justification and commemoration. In their attempt to isolate a problem and define an intellectual project, the editors imposed a chronological strait-jacket on the discussion that inhibited flexibility and lateral thinking. In fact, some of the chapters read like catalogue entries. During the war, loyalty to the imperial cause—be this political or artistic—was, with few notable exceptions, not a problem. It was in the aftermath of the war, when the gap between promise and reality became a yawning gulf, that irony blos-

somed and life beyond moral purpose became for many the goal.

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LAWRENCE S. KAPLAN. *The Long Entanglement: NATO's First Fifty Years*. Westport, Conn.: Praeger. 1999. Pp. xii, 262. \$65.00.

At first glance, this book looks like the kind of unpromising volume that often appears at the time of an anniversary. Articles written in the past, which have lost their contemporary interest, are frequently "cobbled together" and published without any clear rationale. Although most of the chapters in this book have been written for other purposes, Lawrence S. Kaplan has avoided the errors of many other anniversary books: the essays chosen provide an interesting and coherent narrative from the birth of the North Atlantic Treaty Organization (NATO) to the present day. It is also a book that adds to our understanding of NATO as an institution and contributes to the ongoing debate about the future of the organization. As one of the leading NATO historians of his day, Kaplan provides a balanced assessment of the alliance that reflects his conviction that while the world would have been much worse without it (see chapter twelve), NATO has nevertheless been constantly dogged by internal controversy that continues at the present time.

The book is organized into four main sections. The first section contains three chapters that deal with different aspects of the origins of the alliance: the Western European initiative to entangle the U.S. in European security; the importance of the "Atlantic" character of NATO; and the difficulties that had to be overcome before the Mutual Assistance Act of 1949 was passed. In this section, Kaplan shows that the formation of NATO was opposed by critics in the U.S. and was not the inevitable outcome of the Cold War politics of the era. His conclusion, however, is that there is no question that early supporters such as Theodore Achilles and Robert Lovell were right in their judgement that "American involvement was in the nations' self-interest to repel communism by reviving and reconstructing Europe." It also, he agrees, "tapped a vein of altruism in American History that saw a national mission in helping other people to achieve happiness and prosperity" (pp. 1-2).

Despite this, as Kaplan points out in section two, the early years in the alliance's history were fraught with difficulties in reaching consensus among the sixteen allies that rivalled those dealing with the Communist adversary. Disputes over such things as rearmament of the Federal Republic of Germany, Suez, Sputnik, Berlin, and alliance strategy reflected the fact that the European allies were "torn between gratitude for the shelter of American power and resentment over their dependence on that power" (p. 62). These disputes continued, as Kaplan shows, in the second generation

of alliance history from 1968 to 1989. Detente between East and West created more opportunities for differences to be aired: over Vietnam, burden-sharing, the Strategic Defense Initiative and the Intermediate Nuclear Forces negotiations. Despite the differences, Kaplan shows how the alliance held together and contributed significantly to the successful end of the Cold War.

Section four looks at "NATO in the Third Generation" from 1991 to the present. Kaplan demonstrates how the very success of the alliance created new concerns. In particular, the key question the allies have wrestled with since is: what is the purpose of the alliance after the threat has dissolved? Kaplan's approach to this question is interesting. In searching for a new mission, NATO has made a number of errors. Primary amongst these was the precipitate decision to enlarge. In this view, the Partnership for Peace could have been made a genuine substitute for membership. The alliance, he argues, will have to live with the consequences of this decision. The reader gets the impression, however, that Kaplan does not regard the mistake as seriously as writers like George F. Kennan, who see it as the most fateful mistake in Western policy since the end of the Cold War. The key question is whether expansion is likely to become the central cause of new antagonisms with the Russians. Kaplan is very concerned about the consequences of enlargement, but ultimately he comes to the conclusion that it may not be fatal. Despite the difficulties it causes, there remain wider difficulties with the Russians that make the continuation of NATO necessary. In his view, the demise of the Soviet Union and the insecurity emanating from a potentially irredentist Russia "will require the continuation of the NATO umbrella" (p. 237).

The likelihood of the alliance surviving in the medium-term future is the main subject of the last two chapters of the book. Apart from continuing difficulties with Russia, Kaplan also highlights the value of NATO's capabilities in helping to keep the peace in Bosnia in the aftermath of the Dayton Accords of 1995. Kaplan highlights a number of problems and dilemmas associated with alliance peacekeeping operations but concludes that all the members of the alliance recognize that NATO "is the only organization among the many that can provide a military contribution to crises" (p. 219). This also seems to be the lesson from the Kosovo crisis (which broke out after the book was published). At one level, Kaplan seems to overestimate the deterrent power of NATO in relation to Kosovo. NATO, he argues, can "serve to inhibit further outbreaks in such potential points of conflict as Kosovo in Serbia" (p. 218). Clearly this did not happen. In another sense, however, Kosovo does help to reinforce some of the central themes of the book. The crisis was characterized by constant disputes between the allies (especially on the issue of the air versus a ground campaign). Ultimately, however, the success of the military operation reinforced the argu-

ment that NATO remained the only organization capable of dealing effectively with such threats to peace and order in Europe. Despite all the problems with NATO, and the dilemmas it continues to face, as Kaplan points out, there is no sign at present that NATO is becoming either irrelevant or impotent. NATO remains the resilient alliance.

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N. A. M. RODGER. *The Safeguard of the Sea: A Naval History of Britain, 660–1649*. New York: W. W. Norton. 1998. Pp. xxviii, 691. \$39.95.

Given the significant amount of naval history that is written these days, it is surprising to realize that this first volume of a new, multi-volume history of the Royal Navy is the first assayed since William Clowse published the initial volume of his seven-volume study over 100 years ago. The book is nothing if not sweeping, as it begins in 684 with a raid by King Ecgfrith on Ireland and proceeds to 1642. In between, there is a discussion of nearly every battle and campaign by famed or obscure leaders who used sea power to meet their needs to defend or acquire territory, and because the focus is on England, France, Scotland, and Spain figure significantly in the narrative. Woven into the account of sea battles is a constant rumination on the interplay of administration, ship design, weaponry, and money. In a work of such sweep, there are also many interesting facts embedded in the text; in 1588, there were only twenty merchantmen over 200 tons, and the first recorded passage of the equator by an English ship occurred only in 1555. There are also many maps to help the reader navigate the voyages and battles.

The first 700 years of the narrative present little in the way of innovation in either ship design or tactics. The long ship—the premier fighting ship—evolved very little. Merchant ships developed slowly but were the site of greater innovation as merchants sought increased cargo space and more efficient rigging systems. In the long run, the merchant ships would be the testing ground for the fighting ships of the future. As far as tactics were concerned, finding the enemy, always difficult even though hugging the shore was the preferred way of sailing from place to place, led to nothing more than hacking and bashing, as was true on land. As for strategy, N. A. M. Rodger believes that only Richard I and Henry V had any notion of the value of a fleet that could do more than provide protection and supplies for the armies on land. Henry V was particularly cunning in the use of sea power to force his enemies to do what he wanted them to do. As for administration, it was fitful and mostly disorganized. To pay for it all, the crown relied on levies on seaside counties and whoever else could be coerced into paying, including, of course, the London merchant community. More often than not, royal requests amounted to little more than confiscation of resources,

leaving the “donor” with little hope of reimbursement. Rodger excels at showing how the limits of administrative competence also created practical impediments to extended action. For example, only in London was there any hope of putting together sufficient supplies of foodstuffs and barrels to sustain a campaign, and the length of any campaign was determined by the lack of supplies and, of course, the weather.

In the fifteenth century, ship design evolved toward more familiar forms, cannon came on board (of the small personnel type), and England fought the long losing battle to remain a French power. In the sixteenth century, the Tudors assembled something that looked like a Royal Navy. Rodger makes a convincing case that those scholars who date the rise of the navy from the reign of Henry VIII are incorrect. Perhaps there were more modern royal ships and better administrative apparatus, but these still did not a navy make. Only in Elizabeth's reign did a real navy emerge, and Rodger makes the important point that while armies can impose themselves on society, navies need a consensus to survive, as they lack the ability to coerce whole kingdoms. And a consensus was needed in England, which kept its old mixed constitution while European powers became ever more autocratic and, as a result, better able to tax and then use tax money to create overwhelming force. On slender resources and the use of merchant ships, Elizabeth created a navy that was the pride of England. It was a navy that frequently got out of control as commanders avoided carrying out royal orders, preferring to take care of private gain first. It is rather amazing to realize that old habits of piracy could be so compelling in times of mortal danger to the kingdom. But if you had to make public policy by using private means, there were bound to be unfortunate consequences.

For all the achievements of the Elizabethan regime, they were immediately lost to corruption and incompetence in the reign of James I. So pathetic did the navy become that Dutch fleets scoured the English coast to seek out pirates, and North African corsairs raided the English coast. This was a doleful tale that would improve temporarily under Charles I, as he used ship money tax income to rebuild the navy, but even as he did so he was destroying the consensus needed for the support of the navy. His accomplishments were short lived, and the navy quickly fell into disarray in the Civil War. Rodger takes a harsh view of the early Stuart failures and leaves the creating of a truly modern navy to what should be volume two of an ambitious history.

ROBERT C. RITCHIE
The Huntington Library

JAMIE CAMERON. *James V: The Personal Rule 1528–1542*. Edited by NORMAN MACDOUGALL. (The Stewart Dynasty in Scotland.) East Lothian: Tuckwell Press Ltd. 1998. Pp. xvii, 378. Cloth £30.00, paper £16.99.

Jamie Cameron has produced an impressive study of the reign of James V that challenges long-held assertions about the king's cruelty and greed. Unfortunately, Cameron died before converting his Ph.D. thesis into a book. The editor of the series, Norman Macdougall, completed the task.

Cameron clearly focuses upon the years of James V's personal rule with few comments about the preceding fifteen-year minority or about the period of Mary, Queen of Scots (1542–1568). The tight concentration was essential in the thesis stage and allowed for numerous revelations arising from sources such as the registers of the great and privy seals of Scotland. Cameron's ability to tease significance from these and similarly dry legal sources upholds the entire work. Unfortunately, this reign lacks the quantity of Scottish letters, diaries, memoirs, and contemporary histories available for the following one. Cameron makes the point that recent historians have too readily accepted English intelligence reports (especially important for the interpretation of events in 1542), often the only direct source for events, at face value.

The fourteen chapters of the book examine the personal reign from both chronological and thematic perspectives. For example, chapters two, three, four, seven, eight, twelve, and thirteen are clearly chronological. Chapters one and fourteen discuss the image of James V in the eyes of historians (particularly the late Gordon Donaldson; pp. 2, 329, 333–34), and the rest deal with themes, such as the king's policies toward the borders, Highlands, patronage, and the relations of Archibald Douglas, sixth earl of Angus. Cameron does an excellent job in setting the scene of each topic, making the material accessible even to those lacking a knowledge of sixteenth-century Scottish history or the positions of the country's magnates. The biographical material in the text and footnotes on the major players in the king's personal reign will serve as a reference source for future workers in the period. The disadvantage in this abundance of material is a plethora of footnotes, usually over two hundred per chapter. As the book serves as a mine of information on the period, however, the quantity of citations should be accepted.

Previous historians have seen the reign as one of increasing darkness, with the rapacious king milking the church and abusing the nobility, that ended quite naturally in the premature death of James after a humiliating defeat when his nobles went on strike. Cameron challenges these preconceptions and presents impressive evidence to rebut or at least rationalize them. The three final chapters hammer home the point that earlier fixation on the inaccurate and prejudiced English sources has reinforced the negative impression of the king. Cameron's reading of the Solway Moss debacle is one of failure from contingent causes, not disaffection of the nobility (pp. 321–22). He also asserts (p. 333) that the lack of patronage to the nobility (one of the traditional arguments advanced in support of the image of a greedy monarch)

arose from the necessity of establishing authority and the relative youth of those magnates who would have been the natural recipients. Cameron's study, through its use of previously neglected sources, has taken a giant step in the rehabilitation of James V.

Despite the book's generally excellent quality, several problems exist. The author has failed to pay particular attention to either the royal travels or court life that sixteenth-century historians now recognize as vital to creating and cultivating royal prestige. From an analysis of James' hosts, one can probably discover more about his supporters. A deeper look at the court would indicate whether Scotland had begun to import Renaissance ideas of statecraft as it had already begun to do with architecture. Also important to the author's thesis is his failure to look beyond the reign. While Cameron does demonstrate that the king maintained the magnates' personal loyalty in December 1542, he overlooks the events of March 1543, when James Hamilton, second earl of Hamilton, regent, and heir-presumptive, embraced a Henrician reformation of the Scottish kirk. In other words, James V may have obtained the political loyalty of the peers, but he failed to gain their ideological commitment to his policy of maintaining a Roman Catholic Church that could be regularly taxed. Equally, in chapters six, eight, and eleven, Cameron describes royal efforts to overturn traditional patterns of landholding in the county of Angus, replacing them with men obliged to the king. A look ahead would indicate the relative ineffectiveness of the measure. Over the next two decades, Angus became a stronghold of Protestant landholders; the newcomers had little impact in securing Roman Catholicism. Furthermore, of the peers (Angus, Crawford, Glamis, and Gray) targeted there by James, their representatives offered substantial opposition to royal policies during the civil wars later in the century. Thus, royal efforts failed to create a bulwark of loyalists and may have fertilized the ground for vengeance.

Who is this extremely thorough yet controversial book for? Only undergraduates writing on the period will find it appealing. Postgraduates and scholars interested in Anglo-Scottish relations will find it essential. Equally, specialists in early modern Scottish history should examine it, if not for the contents, at least for inspiration in analyzing the drier, mundane legal documents of the period.

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ANNA BRYSON. *From Courtesy to Civility: Changing Codes of Conduct in Early Modern England*. (Oxford Studies in Social History.) New York: Clarendon Press Oxford University. 1998. Pp. 311. \$75.00.

Gesture, oral communication, and "body language" are among the most elusive of historical phenomena, so Anna Bryson has not set out on an easy quest with this work. Most of the sources she employs (primarily

etiquette manuals) exist at several removes from the actual behavior in question and are subject to all the pitfalls of prescriptive literature. In response to this problem, she has adopted the approach of Michel Foucault, particularly that employed in his *History of Sexuality* (1978), treating "courtesy" and "civility" as subjects of discourse rather than actual modes of behavior.

Once she defines her subject as a discourse, she can limit herself to printed, primarily literary works (there are no citations of manuscript sources in this book) and freely admit that the discourse she examines is one concerned only with the behavioral standards of the gentlemanly elite of early modern England. This raises the further problem that most of the etiquette manuals circulating in the period were foreign imports, translations of standard texts such as Erasmus's *De Civilitate Morum Puerilium* (1530) and Giovanni Della Casa's *Il Galeato* (1558). But although these works may have started the early modern discourse on manners, Englishmen made their own contributions.

The most valuable and interesting aspects of Bryson's study are her attempts to correlate changes in the discourse on manners to wider political and cultural changes. "Civility" is related to the eighteenth-century notion of "civil society," but Bryson argues for its earlier association with "urbanity," in the sense of values appropriate for sophisticated urban settings. She contrasts civility/urbanity with courtesy/lordship, the latter being codes of conduct better suited for a rural aristocracy expected to offer hospitality and mingle with social inferiors while still marking its elite status. Although the English aristocracy remained rurally based throughout the early modern period, the growth of the court in the seventeenth century forced noblemen to spend increasing amounts of time in and around London if they wanted access to power and patronage. In the city, removed from the familiar environs of their provincial homes, they encountered many strangers of indeterminate status. Civility required a gentleman to behave in a way that did not denigrate his own standing but also showed respect for the status of strangers. If one were to err, better to overestimate the rank of a stranger than risk insult. Among the effects suggested by Bryson is the near-disappearance of "thou" as a second-person familiar form of address. Everyone, unless obviously inferior or servile, became "you," a more polite label.

Civility/urbanity also required that one display a modicum of learning. Here, the aristocracy and gentry had to tread carefully. Traditionally, their status had come from blood and landed wealth. If the new ethos required that important people appear learned (a spell at one of the universities or the Inns of Court might do the trick), it could not imply that status or power was the result of learning. England could not be turned into a meritocracy. Of course, the Machiavellian emphasis on appearance (of learning, of status, of deference) was not without critics, who saw it as superficial, hypocritical, overly fawning, and even sinful. The

Quakers were only the most extreme example of religious objections to a kind of secular ceremonialism. Playwrights like Thomas Dekker and Ben Jonson were also happy to satirize the exaggeratedly civil "gentlemen" of seventeenth-century London. Eventually, many gentlemen began to chafe under the new code and the cultural shifts that it echoed. Bryson sees the libertinism ("anti-civility") found among some of the notorious Restoration rakes as a last gasp of "gentlemanly transgression" against an increasingly bureaucratized state (p. 275).

This book shows the influence of Mervyn James and his studies of the mentalities of early modern English elites. Because of Bryson's emphasis on "discourse," readers hungry for actual details of behavioral standards may remain unsatisfied, although her third chapter, "The Rules of Civility," does contain some morsels. Much more of such material can be found in Norbert Elias's *The Civilizing Process: The History of Manners* (in English, 1978), criticized by Bryson for inattention to the nuances of class. But fans of a Foucauldian approach to early modern England will find plenty of food for thought in Bryson's work.

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ROY PORTER and G. S. ROUSSEAU. *Gout: The Patrician Malady*. New Haven: Yale University Press. 1998. Pp. xiv, 393. \$35.00.

Roy Porter and G. S. Rousseau, both well-known scholars with a long track record in the history of medicine, have had a fun time with what is essentially a serious and important book about the history of gout, a painful disease caused by the accumulation of crystals of uric acid in the joints. Associated with red wine and rich food, gout has always been thought of—incorrectly—as a rich man's disease. Far fewer women get it. And Porter and Rousseau have employed this disease biography to serve as a mirror of the lives of the rich and cultivated in Britain over the years who have had gout. The authors have exhaustively traced the large medical literature on gout from ancient times to the early twentieth century, when the cause finally became understood. But their book is a literary history as well, and Porter and Rousseau quote to great effect from the letters, diaries, and writings of sufferers such as Tobias Smollett, Edward Gibbon, and Horace Walpole. Thus the book follows two narratives—the objective one of medical analysis and the subjective one of suffering—and makes for a very entertaining, sometimes an enchanting read.

The book's argument is that "the idioms of politics and those of bodily physiology were of a piece" (p. 87). Various medical conflicts about the nature of gout reflected larger political conflicts about the nature of society, they say. The book's fulcrum is probably chapter seven, entitled "Smollett, [William] Cadogan and Controversy," on late eighteenth-century debates about gout and the constitution. The arguments over

gout mirrored the politics of the day: the view that gout was hereditary reflected a certain hopelessness about political change; the opinion that gout resulted from lazy and corrupt lifestyles mirrored, of course, the radical views of politics. The authors find "Old Corruption assailed by reformers, aristocracy threatened by liberalism; indeed, the gout debate is unintelligible unless its politics are foregrounded" (p. 124). This particular claim is a bit much. Are similar debates over tuberculosis equally unintelligible?

Yet Porter and Rousseau charm us into reflecting that some medical debates might serve as larger mirrors of society. And the cultural valence of a given disease—witness AIDS in the 1980s—tells us something interesting about the larger world. The authors find that the valuation of gout slides from positive to negative by the end of the nineteenth century. Gout became an unfashionable illness, perhaps because male invalidism became increasingly unfashionable as a new generation of female invalids grows up. This last observation is my own. Yet it is interesting that in the large oeuvre of Joseph Conrad there is, the authors say, not a single reference to gout.

The book represents the social history of medicine at its best. The field of medical history has been changing dramatically over the past few years, as first-rate scholars such as Porter and Rousseau flow into it and transform it from the antiquarian assembling of dull facts about old doctors into a mirror of society at whose center is the sick person and the doctor-patient relationship, rather than titans in white coats. Even readers who do not have a compelling interest in hyperuricaemia will enjoy this romp through three centuries of British social and literary history.

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RICHARD SHANNON. *Gladstone*. Volume 2, 1865–1898. Chapel Hill: University of North Carolina Press. 1999. Pp. xvii, 702. \$49.95.

In later life and long after his death, William Ewart Gladstone's lofty reputation was unparalleled among Victorian statesmen. His moral stature complemented his achievements as crusading reformer, charismatic leader, and Liberal Party mediator. He was the unquestioned "Grand Old Man" of British political life. This iconic Gladstone has been thoroughly reexamined in recent years. Among the earliest revisionists is Richard Shannon, whose discerning first volume of biography was published in 1982. Shannon rejected the traditional idea that Gladstone's life must be seen as an ineluctable progression from early conservative error to liberal enlightenment. By organizing Gladstone's public life into three discrete "vocations"—the cause of church and state in the 1840s; financial policy in the 1850s; and popular political guide as "the people's William" in the 1860s—Shannon made a convincing case for Gladstone's uneven political development over time. Shannon also gave point to the fact

that Gladstone did not always find his way easily in an era of shifting party allegiances.

Interwoven throughout Shannon's first volume was also a suggestive psychological analysis that additionally undermined Gladstone's reputation as a paragon of moral probity and personal stability. Relying heavily on original sources and on the publication of Gladstone's famous diaries, so skillfully edited by the late Colin Matthew, Shannon found an uncertain and occasionally troubled private individual. Shannon revealed that Gladstone's impulsive actions and hidden passions, combined with his "chronic anxieties" (Vol. 1, p. 211), complicated his struggle to break free from a domineering father and make his own way in the political world. These psychological issues were not merely inconsequential personal quirks: they affected his leadership of the Liberal Party and his standing in Parliament. Widely and favorably reviewed, Shannon's first volume was recognized as an important, if irreverent, interpretation of the younger Gladstone.

Volume two initially seems to follow the themes of volume one. Shannon posits a dominant "vocation" for Gladstone after 1865. Religious in nature, this vocation was characterized by Gladstone's conviction that he was an instrument of the Almighty operating under a "mandate of heaven." The secularized version of this belief was government by executive action or "strenuous government" on the model of Sir Robert Peel, Gladstone's first political mentor (p. 79). This explained why Gladstone often managed his political and party responsibilities by executive fiat. On occasion, he would even withdraw or absent himself entirely in order to avoid consultation with his colleagues and thus act as he alone might wish. Examples include his unprecedented resignation as leader of the Liberal Party in 1874; his secretive behavior during the lengthy Home Rule crisis of the 1880s; and the curious circumstances of his retirement in 1894.

It is probably true that Gladstone was motivated at least partly by his belief in a divinely inspired purpose within the secular world. But to accept this as the sole determinant of his behavior is to take Gladstone wholly at his word. It is equally plausible that Gladstone was also motivated by psychological issues as yet unresolved—issues that Shannon gave significant play in volume one, but which are palpably disregarded in volume two. Shannon's reluctance to address directly the psychological dimension of Gladstone's later career represents the loss of a powerful explanatory tool.

Shannon explains his shift in emphasis this way. In place of the anxious striving and "footloose aggressiveness" (Vol. 1, p. 244) before 1865 when Gladstone was "feeling his way" (Vol. 2, p. xii) as a Christian statesman, he found in the blessings of providence a calmer course and a more certain path after 1865. Yet it is only too clear, based on evidence presented by Shannon himself, that the Gladstone of volume two is very much like the Gladstone of volume one. As leader of the Liberals, for example, Gladstone—because of his "relentless high-minded stiffness" (Vol. 2, p. 22)—was

faced with a Liberal mutiny, Gladstone's loss of temper and bullying tactics, his dictatorial manner, and his unwillingness "to yield a jot or a tittle" (p. 37) further weakened his position in the party. By May 1867, Gladstone was—as Shannon puts it—well into his "normal contentious form" (p. 40).

Attainment of high office did not diminish Gladstone's occasional irascible and violent moods. Shannon quotes substantial evidence of dysfunctional behavior during his first ministry of 1869–1874 that contributed to Gladstone's "frenetic and flailing" image (p. 119). After the defeat of his ministry in 1874, he continued to demonstrate a propensity for "excessive excitability" (p. 147), perhaps most fully manifested during his campaign against Turkish atrocities in the Balkans (p. 207). Gladstone's second ministry from 1880–1885 witnessed stormy cabinets punctuated by Gladstone's frequent threats of retirement. By the time that Gladstone had taken up Home Rule, his alienation from much of the Liberal Party leadership threatened its capacity for effective government.

Of course, there was more to Gladstone than vehement outbursts and unnerving emotional displays. Gladstone often operated efficiently and effectively at the highest levels of politics in a contentious atmosphere for many years, as Shannon notes in good measure in both volumes of his biography. Shannon is particularly good at demonstrating Gladstone's unmatched ability to manipulate political issues by casting himself as the man in the middle, caught not unwillingly between contending parliamentary factions.

Volume two is weakest in its earlier chapters, where Shannon is too often infatuated by the wordy torrent of Gladstone's diaries. But from 1880, Shannon navigates a firm course and his capacity for muscular and incisive historical analysis carries conviction. Overall, Shannon offers us a Gladstone who is thoroughly complex, often infuriating, sometimes mysterious, but never ceasing to command our admiration as he struggled, even in his poignant old age, "toward the light."

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JOHN T. SMITH. *Methodism and Education, 1849–1902: J. H. Rigg, Romanism, and Wesleyan Schools*. New York: Clarendon Press Oxford University. 1998. Pp. ix, 258. \$75.00.

James Harrison Rigg, son of a Wesleyan Methodist minister, was born at Newcastle-upon-Tyne, England, in 1821. His father was an itinerant, but his own ministry was more settled. Twice president of the Wesleyan Conference, he became principal of Westminster College, the London Methodist teacher training institution, in 1868 and remained in the post until retirement in 1903. It was a pivotal position in Methodist education and provided a base from which Rigg was able to participate in important national debates

on the development of elementary education in England and Wales, especially the shaping of policy on religious education.

Despite Rigg's central position in this book, it is not a full-scale biography of him. His son-in-law published a "life" in 1909, the year of Rigg's death, while his daughter prepared a selection of his letters. John T. Smith has had to work hard, and has spread his net wide, to locate manuscript material relating to Rigg. In so doing, he gives us a view of Rigg, man of educational affairs, refracted through the material that surrounds the issues in which he was involved. This moves the book to the heart of the debates of the last thirty years of the nineteenth century over the place of religious education in the English and Welsh primary school system, a system that had been built, with government support, on the voluntary efforts of the churches but that now faced the needs of a developing industrial economy and a society in which the place of organized religion seemed less secure. Moreover, despite Rigg's apparent authority, the ambivalent and changing position of Wesleyan Methodism meant that he did not represent the undisputed voice of his church but belonged to a tradition of conservative ministerial leadership that lay closer to the establishment than those who saw the future of Methodism among the free churches of an England unencumbered by the established church. His position was further complicated by the dimension added to educational debate by what many Protestants perceived to be growing threats: Tractarianism and Anglo-Catholicism within the Church of England, and a resurgent Roman Catholic Church.

It is at the point where Rigg was faced by the need to square his personally conservative instincts with the public discourse surrounding the development of national educational policy that Smith's approach is most challenged by his material. Where they are available, Rigg's papers—such as the "sacredly personal and private letter" to Cardinal Henry Manning that found its way into print in Edmund Purcell's biography of the Roman Catholic leader—add depth and nuance to this portrayal of Rigg's public life. They help to explain his stance as a product of nineteenth-century Wesleyan Methodism and place him in the context of that tradition's rich and varied social and religious life. Smith provides us with a series of helpful appendixes on the state of Wesleyan education, but more material of this kind, integrated into his text, would have provided a context in which to set the rhetoric of the debates that lie at the heart of this book. It would also have helped the reader to understand why a great organization like the Wesleyan Methodist Church ultimately failed to make a contribution to elementary education in England and Wales that was commensurate with its success in covering the land with places of worship.

The issues with which Rigg was concerned had implications outside the national context within which he operated. His North American connections pro-

vided a wider dimension to his work, and it is a pity that the "long series of articles on religion and education" that appeared in the New York *Christian Advocate* do not find a place in the bibliography of this book. Its exploration of Rigg's place in the formation of educational policy in the later nineteenth century is important for the discussion of issues that have considerable general historical significance as well as continuing resonance.

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BARBARA T. GATES. *Kindred Nature: Victorian and Edwardian Women Embrace the Living World*. Chicago: University of Chicago Press. 1998. Pp. xv, 293. Cloth \$55.00, paper \$20.00.

Barbara T. Gates attempts an overly ambitious project that, paradoxically, does not go far enough. Gates has constructed a revisionist "feminist cultural study intended to recuperate and spotlight women's contributions" (p. 5). Unlike contemporary feminist theorists of science such as Sandra Harding or Carolyn Merchant, who "have helped us to question the grounds of a masculinist science perpetuated through a language meant to be unbiased but in actual fact often quite the opposite" (p. 6), Gates wants to capture the fainter voices of earlier women attempting similar challenges. She seeks to reveal how women of the last century treated the mental construct of nature presented to them by men: how they "interrupted, revised, ignored and sometimes disrupted masculine discourse as they participated in conceptualizing, describing, representing, and preserving the natural world of their time" (p. 7). Her text-based study aims to show how women's writings, although sometimes offering only slight variations on men's writings, present distinctively female traditions in science and nature writing. The book compiles brief vignettes of women involved in natural history, early conservation efforts, popular nature writing, hunting, and travel. By lumping high and popular culture together and by surveying artificially connected disparate activities, however, it presents a confusing, and at times confused, picture of pursuits relating to "nature" while failing to follow up on issues that could contribute to our understanding of the role women have played in studying and writing about nature.

Many of the stories will be familiar to historians, for Gates has relied on standard works studying Victorian sensibilities and the place of women in natural science. Studies by Gillian Beer, Harriet Ritvo, David Allen, Robin Doughty, and others are intelligently mined. The marginal roles that women were forced to accept, the formidable obstacles to women who aspired to more serious roles in natural history, and the natural alliance between suffragettes and antivivisectionists are well illustrated. Gates has an eye for engaging accounts, and readers who find the feminist theoretical underpinnings of the book tedious will enjoy her portraits of several too-often-overlooked Victorian

women. By casting her net so wide and including everything from fossil collectors to avid game hunters, however, the original female voices in nature writing get lost in a cacophony of diverse and only tangentially related activities.

Gates correctly intuits that Victorian women contributed to natural knowledge and contributed to cultural perceptions of nature. Her study unfortunately stays at too superficial a level to extend that insight. For example, the discussion of Elizabeth Gould tells us that her husband relied on her considerable artistic talents for his early bird books. John Gould, however, continued to rely on other collaborators (men) after Elizabeth's death. What exactly distinguished Elizabeth Gould's illustrations? Conceptual confusions, such as equating conservation with ecology, also mar several of the discussions.

As a tableau of women involved in a wide array of endeavors relating to nature, this book presents an interesting picture. Gates's aim, to capture distinctive feminine perspectives, fails, however. Opportunities to explore unique contributions get by-passed in favor of describing ever more characters. Historians are likely to be frustrated by the admixture of chapters from different narratives and by the superficiality of the alleged links between women writers of the past century and contemporary movements like ecofeminism. Quiet distant voices do not equate to historical links, at least, not without a convincing historical investigation to demonstrate them. Gates does tell her readers that she primarily intends her book to provide starting points for further inquiry, and in that sense it raises numerous potentially valuable questions.

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DAVID CANNADINE. *The Rise and Fall of Class in Britain*. (University Seminars/Leonard Hastings Schoff Memorial Lectures.) New York: Columbia University Press. 1999. Pp. xv, 293. \$29.95.

David Cannadine likes things to fall. After *The Decline and Fall of the Aristocracy* (1990) comes this new book on the rise and fall of class. Unfortunately, his lyrical titles do not always reflect their subjects. In the earlier book, it was not entirely clear that the British aristocracy had fallen, although the exclusion in 1999 of most hereditary peers from the House of Lords suggested that at least Cannadine was pointing his readers in the right direction. With this book, the fall is even less obvious, since Cannadine concludes that Britain is still a class-bound society containing numerous obstacles to the classless society for which modern politicians yearn. Just as the aristocracy are very much with us in the new millennium, so "class" remains at the heart of much British social organization. Cannadine's book is not about the rise and fall of class; rather, it is a coherent and sustained post-Marxist analysis of the nature of class society since the eighteenth century. It is both a critique of the approaches adopted by

historians of class and a sophisticated discussion of why Tony Blair's Britain remains obsessed by class when predictions of its demise have come and gone with remarkable regularity throughout the twentieth century.

Cannadine offers an interpretation of the British class system in terms of three interlocking and often overlapping models. The first is the hierarchical seamless web of status groups and ranks. The second is the triadic division of upper, middle, and lower collective groups. The third is the adversarial picture of "us" and "them." Cannadine argues that these descriptions go back to medieval times but are most useful as a means of describing class in Britain since the eighteenth century. Karl Marx, and the social historians who have followed him in the twentieth century, believed that class structure and class analysis provided the key to understanding British history and modern British life. Cannadine is not so sure. Instead, he argues that the three models enable us to escape from the consciousness and conflict of Marx's terminology toward understanding class as a means of social description and social identity.

The heart of the book lies in three chapters covering the eighteenth, nineteenth, and twentieth centuries, respectively. By the third quarter of the eighteenth century, the language of class was already common currency, and each of the three models of class can be found expressed in the contemporary literature. Consequently, the 1780s to the 1840s, identified by Marx as critical in the making of class, were important not because of new social structures but because the existing models were described and discussed with unprecedented urgency, intensity, and anxiety. Cannadine accepts that in these years the British social order was wrenched and transformed by a succession of seismic shocks as a result of the fallout from the French Revolution, the agricultural and industrial revolutions, and the rapid growth of population (and cities). Discussion was never greater than over whether or not the chief victors of the 1832 Parliamentary Reform Act were the middle classes. Yet it became clear in the 1830s and 1840s that the aristocracy had *not* been vanquished, and the middle class had *not* risen triumphantly. For Cannadine, it was the language of class rather than class itself that was invented in these years. Most Victorians, he argues, accepted that they lived in a hierarchical society.

Subsequently the three models continued to be found in contemporary discourse. Cannadine traces the story through the late Victorian and Edwardian period into the interwar years and then through the 1960s and 1970s. He reserves special attention for the contribution to class understanding (or not) of Margaret Thatcher and adds a few thoughts on the classless society envisaged by Prime Ministers John Major and Blair.

The problem with Cannadine's argument is that class is reduced simply to a descriptive ideal-type model that lacks the resonance of conflict. Cannadine

is aware that he is in danger of turning British society into a rather bland and boring social construct: "this book has not been entirely about unchangingness," he writes; "each of the three models of British society that I have discussed carry with them an implicit temporal dynamic and perspective that is part of their appeal (p. 174)." Class is an emotive issue in Britain today because it implies division between the haves and have nots, between the rich and the poor. This is the dichotomous relationship claimed by Cannadine as one of his three models, but it would be wrong to see it as a bland construct. Modern Britain may not be in striking distance of a revolution, but the class divide still determines the way in which people live and their perceptions of society. Class has not fallen, but even so Cannadine has written a provocative book. It will annoy as well as enlighten, but it offers an interesting reappraisal of class in Britain over the past three centuries.

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ROSS MCKIBBIN, *Class and Cultures in England, 1918–1951*. New York: Oxford University Press. 1998. Pp. x, 562. \$45.00.

This book by Ross McKibbin was intended to be part of the New Oxford History of England, but it does not provide the broad synthesis expected of volumes in that series and, presumably for that reason, it has been published as a free-standing work. Although it contains a detailed social and cultural history of England from 1918 to 1951, it ignores high politics (and high culture) and diplomatic and imperial history. England was one of the world's most working-class societies during these years, and this book is primarily about that class and its culture.

McKibbin's study has two parts. The first, on social structure, includes separate chapters on the middle and upper classes, followed by two on the working class. In each, McKibbin traces the development of that class from the early 1920s to 1951. The discussion of the remaking of the Edwardian middle class during the interwar period is especially valuable. The second part contains eight chapters on major cultural forces that shaped class behavior: education, religion, sport, music, cinema, radio, sexuality, and language. Although D. L. LeMahieu examined many of these topics for the interwar period in *A Culture for Democracy: Mass Communication and the Cultivated Mind in Britain between the Wars* (1988), McKibbin extends the discussion to 1951 and places the cultural developments into a more explicit class context.

But it would be misleading to imply that McKibbin examines class and culture in separate compartments as if they were unrelated. His main concern is with the interaction of class and culture. Although he demonstrates the profound effect of class on English cultural life, McKibbin's underlying purpose is to reveal the importance of culture in shaping working-class politi-

cal behavior. His exploration of social and cultural history is intended to answer a political question: why did those who had authority in 1918 still have it in 1951, despite the intense class feeling of the 1920s and World War II's recasting of social relationships?

Implicit in McKibbin's choice of dates to begin and end his study is a conviction that class consciousness reached a peak from 1918 to 1951, and that this distinguishes the period from preceding and following decades. Rejecting recent studies that have minimized the importance of class feeling in the aftermath of World War I, McKibbin claims that England experienced the most intense class conflict of any decade in modern English history during the 1920s. Given this intense class antagonism, he attempts to explain why the working class could not sustain the class consciousness necessary to make the Labour Party dominant politically. He concludes that working-class culture explains why nearly half the interwar working class voted Conservative.

Rejecting the view that World War II promoted social harmony, McKibbin argues that by reviving class feeling it radicalized the working class, thereby creating the opportunity to transform English society. But this opportunity was lost when the Attlee governments restricted their reforming efforts to social welfare and the nationalization of selected industries. The cultural institutions that shaped working-class attitudes, from sport to education, remained unaltered, thereby allowing the upper classes to reestablish their dominance. Underlying McKibbin's discussion is a sense of nostalgia for a missed opportunity to democratize English culture that may never emerge again.

Although McKibbin focuses on class, he portrays gender as being nearly as crucial in shaping perceptions and behavior. In almost every chapter, he draws attention to the importance of gender and stresses that working-class women's experiences differed significantly from those of working-class men in private as well as in public life. Gender also influenced working-class voting patterns. Within the interwar working class, women were more likely than men to vote Conservative.

McKibbin's book builds on his earlier study of class ideology (*The Ideologies of Class: Social Relations in Britain 1880-1950* [1990]). But this is a much more ambitious volume; the implications of class culture for the period have never been spelled out so thoroughly. The strength of this book lies in its detailed description of how class and culture interacted. Some readers may also consider this to be its weakness; in some chapters, the author's themes are obscured by the lengthy description of social and cultural institutions. Although he acknowledges the importance of anti-Irish prejudice in shaping Liverpool working-class politics, McKibbin pays little attention to regional and ethnic differences within the working class. But these observations do not detract from a work of major importance that will have to be taken seriously by anyone

concerned with the role of class in twentieth-century England.

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SEAN O'CONNELL. *The Car and British Society: Class, Gender and Motoring, 1896-1939*. (Studies in Popular Culture.) New York: Manchester University Press; distributed by St. Martin's. New York. 1998. Pp. x, 240. Cloth \$79.95, paper \$29.95.

E. M. Forster registered early disdain for the motorcar. It is the symbolically appropriate conveyance of those bourgeois philistines, the Wilcoxes of *Howards End* (1910); it oozes oil on the driveway and runs over a cat. By 1939, however, what remained a vulgar, polluting, lethal machine to intellectuals had become the golden calf of an expanded modern lifestyle in Britain, where one in five families were now car owners. Historical studies of the car in Britain (unlike in the United States) have been restricted to the prosaics of its manufacture or the heroics of its leading engineers and entrepreneurs. In this admirably well-researched and well-argued pioneer study, Sean O'Connell moves away from technological determinism to the cultural politics of the car, investigating its impact on leisure patterns, class and gender identities, and issues of public good and private interest.

Car owners in these years were predominantly middle class, car drivers predominantly male. Cars registered fine degrees of style and status in the differentiation of makes and models. Learning to drive became a rite of passage for the young male, while the car was fetishized as an exclusively masculine accessory, to be defended against any "feminizations" of design or the encroachment of women drivers declared biologically ill-equipped to take the wheel. The car and the new mobility it conferred were employed more for leisure than for work, expanding the range of modern "middlebrow" pleasures, from touring to drinks at the "roadhouse." From the start, however, social and other costs were severe: traffic jams, pollution, breakdowns, high running expenses, frequent theft, disputes over policing and regulation, and a steep increase in road deaths and injuries. O'Connell reveals how a powerful private motoring lobby displaced the responsibility for accidents on various scapegoats, notably the negligent pedestrian. Road safety therefore meant educating those almost literally out of step with modern progress rather than enforcing tighter controls on the car and its driver.

O'Connell effectively demonstrates the constant conflicts and ambiguities generated by this supposedly benign juggernaut. For example, the car brought the countryside within easier reach of the middle-class excursionist, but the increased traffic threatened the peace and quiet that was his quest. Conservationists rushed to protect a newly idealized rural England, but among those they sought to protect were country people who added to the blight by opening unsightly

petrol stations and cafes to service the carborne invaders. Meanwhile, the urban working class relieved any ache of relative deprivation in an epidemic of joy-riding, although some skilled workers in the car industry became legitimate consumers, reducing the expense by sharing ownership and doing their own maintenance.

A concluding chapter explores the potency of the car as sign in a variety of representations. The illustrations in the book are too meager to demonstrate the impact of visual imagery, while the rich contemporary advertisement on the cover goes lamentably unremarked in the text, but O'Connell successfully conveys the frisson of quasi-licit deviancy that the car's complex charge of allure and menace imparted to the popular literature of crime and sexual romance. Such interpretations suggest further opportunities for historians alert to the psychic and symbolic undertow in a culture still too readily accepted at its own dispassionate face value. As the social history of interwar Britain expands, there are fruitful connections to be sought between explorations of the social imaginary pioneered by Alison Light and others and the cultural materialist synthesis offered by Ross McKibbin. Though not always signposted as such, this is the road that O'Connell travels, and travels well.

The author urges similar attention to the later cultural history of the motor car, but the agenda should not be limited simply to bringing the story up to date. The car needs studying within a larger history of the changes wrought by an increasingly democratized world on wheels, from the railway on. More could also be done to understand the ambiguous role of the car in constructing a consumer consciousness at once collective yet highly atomized. This has wider social and political implications. Once he learned to drive the latter-day Good Samaritan was less likely to stop and succor social casualties of any kind.

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JOHN KENDLE. *Federal Britain: A History*. New York: Routledge, 1997. Pp. xiii, 204. Cloth \$69.95, paper \$22.95.

This book's title might suggest that famous non-title for a German monograph, "On Snakes in Ireland," but John Kendle demonstrates, covering a period from the early 1600s to the present day and using a wide range of sources, that the federal idea, in some form or other, has been present for an unexpectedly long time in British constitutional debate. When Anthony Eden famously said, à propos federalist aspirations of the early European Economic Community, "the federal solution is not possible for us," he was forgetting that his colleagues at the time, and generations of Whig or Tory statesmen before him, had thought federalism was just the thing for others: Canada, Australia, Nigeria, and the ill-fated East African Federation of the 1960s.

Kendle clearly shows, however, that much of the talk

in the 1890s and 1900s about "Imperial Federation" in the Round Table "movement" (or circle, rather) was not really federalism at all but simply colonial representation in a Westminster imperial parliament that would still remain sovereign. The confusion of "sovereignty" with real power was endemic in the debate about holding and strengthening the empire, and about conciliating Ireland. True federalism would mean the break-up of the United Kingdom and a diminishment of British power in the empire. Kendle quotes one advocate of closer imperial union in the 1890s as saying frankly that the word "federation" was used by imperial unionists "simply because it is the most convenient word we can find to represent the idea we have in view—the idea of some organization which shall efficiently give us what we want to maintain—the strength and security of the empire" (p. 48). Others more logically maintained that "In so far as an institution is Imperial, it cannot be Federal, and in so far as it is Federal it cannot be Imperial" (p. 51).

But theory ever takes second place to practice in British thought. The word "federalist" is, after all, more likely to warm the blood—slightly—than is "devolutionist." But it is another question, beyond this excellent book, as to whether an extreme devolution of powers, such as in the Scotland Act (1998), is likely to lead to so much administrative and legal confusion, that formal federalism might not gradually emerge. Kendle notes that the Royal Commission on the Constitution in 1973 used the term "quasi-federalism." In fact, an American political scientist long resident in Britain, Richard Rose, first made this term current: Rose was himself a firm unionist, both in political terms and theoretically. Kendle amply demonstrates the extraordinary tenacity of the British belief that federalism is somehow inherently unstable, and that the doctrine of sovereignty ensures the effective strength of central government. But I think those of us, like Rose, who use the neologism "quasi-federalism" do so to point to the intense political difficulty of reclaiming important devolved powers once granted. For the limitations of the use of sovereign power are what count politically, and what can be understood historically. Kendle notes that most British politicians, not only William Blackstone and the common lawyers, "were purblind" to the quasi-federal nature of the first British Empire, which was best governed, as Edmund Burke saw it, "by a wise and salutary neglect."

Strictly speaking, William Ewart Gladstone's Irish Home Rule Bill of 1886 was not federal in form, but both its opponents and its supporters were agreed that once legislated, it would be hard to imagine circumstances in which it could be taken back in the way the old Irish Parliament had been abolished in 1800. In the 1840s, Lord John Russell had resolutely opposed Daniel O'Connell's efforts to promote a federal solution, American style, to the Anglo-Irish problem; but he lent his support to Charles Grey's efforts to create federations in both Australia and British North America. That was, became, and still is par for the British

course. Kendle has written one of those rare books that locate the episodic history of an idea firmly in the context of the contemporary politics. He states very early on that "the American federal system . . . was born out of practical necessity and did not rest on any full-blown federal theory" (p. 18). If federalism comes to the United Kingdom, or to Europe for that matter, it will not be because of a belief in "federalism" (or, as Harold Laski once intoned, "all power in federal") but as the only way to solve great practical difficulties.

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SUSAN PLANN. *A Silent Minority: Deaf Education in Spain, 1550-1835*. Berkeley and Los Angeles, University of California Press, 1997. Pp. xvi, 323. \$40.00.

The tradition of deaf education is often traced to what is believed to be the earliest published work on the subject, Juan Pablo Bonet's *Reducción de las letras y arte para enseñar a ablar los mudos* (1620). As Susan Plann explains in meticulous detail in her book, Bonet himself had little direct experience with deaf people. Plann describes Bonet's account of the methods of educating the deaf as most likely plagiarized from Manuel Ramírez de Carrión, a tutor who had been employed to teach Luis de Velasco, the deaf son of the sixth constable of Castile, Juan Fernández de Velasco y Tovar. As secretary to the head of the Velasco household, Bonet lived in the household at the time de Carrión was employed. Furthermore, de Carrión in turn probably borrowed heavily or entirely from an earlier tutor, the Benedictine monk Fray Pedro Ponce de León, who had also taught de Velasco's deaf sons. Like de León, de Carrión did not record his successes, leaving Bonet, after only a brief experience with de Carrión's young charge, to write what is considered one of the earliest and most detailed accounts of how to educate the deaf. Bonet's role is at the heart of Plann's skillfully written account of deaf education in what she calls the "preprofessional era," from the middle of the sixteenth century through the start of the nineteenth century, when education was carried out individually and selectively, that is, for the privileged and wealthy few.

Plann traces the start of Spanish involvement in deaf education to a time of epistemological shift, occasioned by the appearance of writings by Renaissance thinkers such as Girolamo Cardano and Rudolph Agricola, who suggested that learning and thinking could take place "by eye" and without speech. Although there were others who argued that speech was the likely vehicle of the intellect, the novelty of ideas about the separation of speech and thinking most likely circulated among monasteries in Spain and thus eventually reached de León. Fray de León undertook the teaching of speech to de Velasco's two sons as his most urgent duty, since, as Plann explains, de Velasco was anxious that his sons be able to inherit and the

family's holdings remain intact. The courts would be persuaded of their fitness as landholders if they were able to speak Spanish as well as to read and write it. Thus began what Plann describes as the first task of deaf education in Spain: the preservation of wealth and privilege.

Although de León and later Bonet received a large measure of contemporary recognition for their work, Plann tempers claims of their achievements. As she sorts through records of their lives, where they were born and how they came to enter the employment of the wealthy families, she digs deep into their motives. Plann suspects that the unusual absence of information about de León's family suggests he was most likely of illegitimate birth and lost privilege. By teaching the children of the nobility, de León found himself invited to enter the most privileged of spaces that would otherwise be denied him—the homes, offices and courts of the rich and landed in Spain. As for the secrecy he built around his methods, de León was motivated by a fear that his methods would be stolen and his livelihood (and the recognition that came with it) would disappear. De Carrión likewise found he could command his price as long as he was the only available tutor. Bonet did little if any teaching himself; instead, he counted on his writing to gain him acclaim. Plann describes Bonet as an opportunist; he designated himself first author because no other before him would. Instead of portraying these men as benevolent teachers, Plann finds that they were allowed almost complete freedom in how they accomplished their teaching, which included, in some cases, harsh treatment and the development of psychological dependence in their young charges.

Plann scours this early history of deaf education for evidence that de León might have used sign language with his students, or that the deaf students themselves signed and formed a community with other deaf individuals. De León's students all had deaf relatives, either siblings or cousins as well as aunts and uncles, so the possibility of coming into contact with other deaf people was almost certain, but whether this small elite number of deaf nobility signed or formed communication alliances with each other is not indicated in the records of their education. Plann speculates that there is a good chance the deaf nobility signed, because recent research suggests that whenever small groups of deaf people are found in closed societies, a sign language is used by them as well as by hearing relatives or community members. Plann also speculates that the Benedictine monks might have been the source of at least some of the gestures that the deaf used among themselves. The monks, including de León, resided at monasteries where the practice was to refrain from speaking by voice during most of the day; instead the monks used gestures for necessities and basic activities. Quite possibly these gestures were adapted by de León for use with his deaf students, but there are very few records referring to any signing. Indeed, throughout the nineteenth century, the signing behavior of

deaf people is recorded sparsely and distantly. Instead, as Plann shows with example after example, early records of educating the deaf are largely about how to succeed at teaching articulation and vocalization to individuals who cannot hear themselves speak. There was no attempt to record where and how deaf people lived, how they met each other, or how they engaged in communication and perhaps even commerce with each other.

By the end of the eighteenth century, the ideas of Enlightenment began to take hold throughout Europe and sentiment shifted to favor educating a broad class of deaf children. Spain began to consider establishing schools for deaf children, following the lead of France, Italy, and Germany. Here Plann addresses a key question in deaf history: why did some of the earliest schools recognize and incorporate sign language while others refused, instead insisting on an oral pedagogy? Her answer is nicely textured as she moves between the history she has uncovered and histories written by other historians of deaf education, including Harlan Lane, John Vickrey Van Cleve and Douglas Baynton. The establishment by the Abbé Charles-Michel de l'Épée of the first public school for deaf children in Paris around 1795 is taken to be a watershed in deaf history; the school became a stable core around which a deaf community could develop. Graduates of the school moved outside the walls of the institution, took jobs in the community, and continued their association with friends and schoolmates. Lane has argued that de l'Épée's achievement was to recognize the existing gestural usage of the deaf children who came to the school and to incorporate it into his method of education. Plann points out that Bonet, however, made only one reference to signs used among the deaf in his entire volume. Instead, he saw the role of the educator as insisting on speech and on the "total exclusion of signs in communicating with deaf people," a view of "intransigence" that Plann describes as the "cornerstone of oralism" (p. 49).

Spain's entry into public deaf education followed that of France by a number of years and was in fact accomplished by a Frenchman, Antoine-Joseph Rouyer, who was born in Paris, but spent most of his life in Spain. He returned to Paris to attend university there and became acquainted with de l'Épée's successor at the Parisian school for deaf children, the Abbé Roch-Ambroise Sicard. After studying Sicard's methods, Rouyer returned to Madrid and petitioned a philanthropic organization, the Friends of the Country, to support his proposal for a school. The shift from individual privileged tutoring to education on a larger and common scale such as that proposed by Rouyer represented a significant shift for Spain, as monasteries ceased to be sites of education and the state began to take over the function of educational management, albeit through a crown-sanctioned philanthropic organization such as the Friends of the Country.

But the state's assumption of responsibility for the education of deaf children proved to be tenuous,

indifferent, and, at critical times through the nineteenth century, nonexistent. By the time the War of Independence ended in 1813, the crown's support of the Royal School for Deaf-Mutes in Madrid had declined to such trivial amounts that its students were reduced to starvation and begging in the streets. Its employees ceased to be paid, and the head teacher lost his post. As Plann traces in exhaustive detail the changes in management of the school through these disruptive times, she shows how sudden departures and political replacements pushed the school toward a nationalist stance. When it reopened in 1814, its new head rejected the French methods and insisted on a return to Bonet's system, a decision that Plann describes as "more political than pedagogical" (p. 158). Thus education in Spain returned to its roots in Bonet's methods, a philosophy that would largely remain in place in state-supported schools throughout the twentieth century.

In her epilogue, Plann addresses a number of contemporary questions, but not the question that preoccupies many of us who study sign language and deaf communities. As America and Europe both experiment with moving away from nationally organized deaf schools toward local, deinstitutionalized management of deaf education, could we be returning to the fragmented and dispersed communities of the type that characterized Spain before the founding of its deaf schools in the early nineteenth century? Plann is appropriately restrained about comparing without context, but this is a question that one cannot help but ask after reading her splendidly written history.

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TIMOTHY MITCHELL, *Betrayal of the Innocents: Desire, Power, and the Catholic Church in Spain*. Philadelphia: University of Pennsylvania Press, 1998. Pp. 178. Cloth \$39.95, paper \$16.50.

Combining the best of history, sociology, and psychology, Timothy Mitchell is well known in Hispanic studies for eclecticism, insightfulness, and originality. In four previous books, he has illuminated the nooks and crannies of familiar Spanish institutions like the bullfight, Holy Week, and flamenco music as well as providing diverting and witty overviews that make one think in fresh and exciting ways. The present book continues in this vivifying tradition, tackling the problem of ecclesiastical misconduct and anticlericalism in Spain, mainly between 1870 and 1936.

In fearlessly tackling this subject, Mitchell has done signal service by opening the matter up to a new debate, which will be, one hopes, free of the political passions of the past. It is indeed curious that this important topic has not been adequately investigated previously. Spanish anticlericalism was virtually unique in the extent to which its protest took on an erotic coloration with chiliastic overtones. Anti-church fanatic-

ics between 1870 and 1936 were obsessed with the sexual peccadilloes of priests, and the entire focus of their perfervid condemnation for many was directed at the clergy as sexual predators who supposedly used clerical power to pollute the entire body politic, corrupting both boys and women. One Spanish novelist of the time, Vicente Blasco Ibañez, called priests "sexually-perverted sadists," a view apparently seconded by popular writers like Benito Pérez Galdós, Felix Trigo, and José Martínez Ruiz Azorín, not to mention politicians of the Republican period like Miguel Azaña, whose fiery tirades against the church were clarion calls to the Francoist Right and helped spark the violent religious wars of the 1930s.

Mitchell's argument is that the well-documented sexual hypocrisy of the priests, what he ironically refers to as the "celibate sexual power system" (which was tacitly accepted by the Catholic hierarchy), gave rise to such a volume of routine abuses that a large minority of the population turned violently against both clergy and the church itself, thereby exacerbating the revolutionary fervor of the Left and leading to the murderous explosions of church-burning and persecution. It was this priestly sexual predation, for example, that led directly to the "anticlerical fury" of 1936 (p. 86), when thousands of clergy were murdered and of hundreds of churches desecrated or burned by mobs. Defense of the beleaguered clergy was a rallying cry of the Nationalists, and so one may rightly say that the sexual misconduct of the priests was a principal cause of the Civil War.

Mitchell supports his erotic-political argument with an extraordinarily complex mass of popular data: trial reports of rape and sexual abuse, newspaper and eyewitness accounts, as well as a host of literary sources ranging from poetry and novels to theater. In compiling all this documentation of clerical sado-masochism and pedophilia, the book takes a strongly psychoanalytic turn, citing the work on sexual perversion and repression by neo-Freudians such as George Devereux, Lloyd DeMause, Elizabeth Young-Bruehl, Giles Deleuze, Robert Stoller, and many others. Mitchell's insights into the damage done by "authoritarian sexuality" and what he calls the "hysterical/hierarchical" mindset of the Spanish Right make for arresting, and sobering, reading.

Another strong point book is the discussion of the effect of sexual repression on Spanish women, the shape of Spanish misogyny, and the woman-in-the-street's reactions to church doctrine. Mitchell shows how varied the responses of Spanish women were, from the anticlerical rage of the women on the Left like *La Pasionaria*, who was said to have ripped out a priests' jugular with her teeth, and the pious maidens of the Right who defended their church with equal violence. I do not believe this subject of female ardor has been brought to light previously with such elan.

Pulling no punches, timely, scholarly and superbly written, Mitchell's book is the definitive work on the religious dimension of the "Two Spains." He is to be

congratulated once again for the masterful way in which he has blended history and anthropology. My only quibble is that at times Mitchell appears to be taking sides with the anticlericals and letting the misconduct of a few serve for a blanket condemnation of the entire clergy. I would have toned down this negativity a bit. Still, the book is a watershed that should remain the touchstone for future work on anticlericalism in Spain or anywhere in the Catholic world. Bravo!

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JAVIER UGARTE TELLERÍA, *La nueva Covadonga insurgente: Orígenes sociales y culturales de la sublevación de 1936 en Navarra y el País Vasco*. (Colección Historia Biblioteca Nueva.) Madrid: Biblioteca Nueva, 1998. Pp. xii, 478. 3,500 ptas.

Javier Ugarte Tellería's richly woven regional study of right-wing popular mobilization in the summer of 1936 is a welcome addition to the literature on the origins of the Spanish Civil War. Within a narrow geographical and chronological frame, Ugarte plunges deeply beneath the surface of the events leading up to the anti-republican uprising of July 17th, 1936. As he describes his project, he wanted to write not a history of the war but a history of the society that made the war possible (p. 3). This quest led him into a multilevel investigation of social networks, urban/rural relationships, ideologies, and organizations, community ethos, symbolic liturgies and the encounters between all these social realities. The result is a powerful analysis of why many people in the provinces of Navarre and Alava rose against the republic in July of 1936.

According to Ugarte, the simple answer to this question is that the symbology and ideologies of anti-republican politics connected with what he calls the ethos of a large sector of the population. What Ugarte seeks to flesh out in the book is the encounter between a community ethos, comprised of values, social networks, and a sense of common identity, and the more cognitive cosmovision (he borrows these terms from Clifford Geertz) that ascribed political meaning and direction to the "logic of everyday life." In his telling, the Navarrese enthusiasm for the nationalist cause was rooted in a complex set of associations that had less to do with simple ideological adhesion and more to do with how the right-wing forces were able to root themselves in community institutions and values. Drawing on extensive oral interviews as well as conventional archival sources, Ugarte brings these associations alive with individual stories.

Although Ugarte's analysis of the specificities of Navarrese political culture is fascinating, what is more compelling is his powerful comparative framework. Throughout the text, he demonstrates his engagement with a broader literature on the interwar crisis and the rise of fascism, and every result or conclusion is held

up to comparative scrutiny. The larger goal is to situate Spain in the broad European crisis of liberalism that unfolded in the late nineteenth and early twentieth centuries and thus to slough off the remaining air of "difference" or "backwardness" that still clings to Spain in some of the historiography. More specifically, he portrays Spain as simply another case study of a society in which right-wing forces built new coalitions as a response to the demands and tensions of the transition to a mass society. What was different in Spain, as in each individual country, was the particular balance between conservatives and what he calls "movement" forces within the right-wing coalition.

Central to Ugarte's effort to integrate Spain into the larger framework of right-wing coalitions is his treatment of the Carlist movement, the backbone of the popular anti-republican uprising in Navarre and Alava. Carlism originated as an ultramontane movement in the early nineteenth century, rising in defense of absolutism, religion, and local rights. As a result, its role in the 1930s has often been dismissed as an anachronism, a vestige of past wars between liberalism and absolutism. What Ugarte argues is that Carlism should be viewed as an authentically "modern" mass movement, which sought, like the other "movement" cultures of Europe, to mobilize the masses through appeals to ultra-nationalism, authoritarianism, and mythic emotional symbols. By deemphasizing its "traditional" ideology and focusing more on the way it functioned, Ugarte puts Carlism on the "movement" side of the right-wing coalition, along with Nazis, Italian fascists, and the Spanish Falange.

Once Carlism is viewed from this perspective, Spain's right-wing coalition looks more complex than it is often portrayed. Thus, the Spanish "military coup" is usually contrasted with the Italian and German situations, where new regimes came to power through the combined effort of conservative and radical paramilitary forces. As Ugarte says in his conclusion, he wants to push beyond the "military coup" theory of origins, and demonstrate that the Franco regime came to power through an equally complex web of negotiations and compromises between conservatives and movement forces. That the military proved the dominant force in this coalition does not, in Ugarte's mind, discount its complex origins. Nor should it disqualify the Franco regime (before 1945) from its place along the spectrum of "fascisms" (for lack of a better term, as he puts it).

Ugarte's classification of Carlism and the conclusions he draws from it are likely to be controversial, since they feed into the ongoing debate about the nature and definition of fascism. I was certainly convinced by Ugarte's argument that Carlism in the 1930s functioned as a new mass movement and not as an anachronistic throwback. And yet, the genealogies of Carlism and the fascist movements in Italy and Germany seem distinct enough to disturb general theories of the origins of fascism. Most notably, as Ugarte points out, Carlism emerged not out of the anomic and

alienation of mass society but out of the strong clientelistic and communitarian bonds of a close-knit, largely traditional society. Creating a framework that links the uprooted ex-soldier of Italy's squads and the devout subsistence farmer in Navarre would require some rethinking of the existing categories and narratives. In any case, Ugarte's book has offered a provocative and compelling place to start.

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GEORGE HUPPERT. *The Style of Paris: Renaissance Origins of the French Enlightenment*. Bloomington: Indiana University Press, 1999. Pp. 146. Cloth \$35.00, paper \$14.98.

In this short, sprightly analysis of the French Renaissance, George Huppert argues that the diffusion of the humanist program of education from 1520s Paris to the rest of the kingdom created a new style of thought—the "style of Paris"—that would later become the defining characteristic of the French Enlightenment. Most broadly, this style rejected all forms of slavish obedience to religious and intellectual authority and embraced the principles of religious toleration, freedom of thought, and social equality. Huppert presents this argument through a series of vivid sketches of lesser-known figures, like the naturalist Pierre Belon and the youthful man-of-letters Jacques Tahureau, and of better-known ones like Petrus Ramus, Étienne de La Boétie, and Étienne Pasquier. The ideals of these philosophes, as Huppert terms them, continued to spread throughout the seventeenth century, winning new converts in the "war against ignorance" (p. 114), until that war was finally won in the French Enlightenment.

No one can deny the pronounced affinity between Renaissance and Enlightenment. But in arguing for the "Renaissance origins of the French Enlightenment," Huppert goes beyond mere claims of "influence" to stake out a much stronger position: that the central tenets of the Enlightenment sprang from the brow of sixteenth-century philosophes. What we call "the Enlightenment" simply marks the point at which sixteenth-century ideals, propagated throughout the seventeenth century by a system of public education, gained ascendancy.

In an academic world dominated by big books making small claims, it is refreshing to encounter a small book making big ones. On this account, we can excuse some of the inevitable lacunae in Huppert's argument, especially concerning the details of the seventeenth-century diffusion of the "style of Paris." But other lacunae lie much closer to the core of his argument and cannot be so easily excused. For example, if the Enlightenment actually originated *in* the Renaissance, how can we distinguish it *from* the Renaissance? For Huppert, Renaissance humanism *per se* was marked by "ritual genuflections in the direction

of Athens and Rome" (p. 48), whereas those whom he calls sixteenth-century philosophes had moved beyond the slavish imitation of antiquity. This position reflects a misunderstanding of the central feature of Renaissance humanism, *imitatio*. The highest form of imitation was emulation, whereby the humanist had so thoroughly digested ancient models that he could push off against them, willfully transcending them to establish his own originality. What Huppert identifies as one of the principal features of the "style of Paris"—the freethinking rejection of authority—is nothing other than the most sophisticated form of Renaissance *imitatio*.

How could Huppert have so fundamentally mistaken Renaissance for Enlightenment? This question leads to another serious (and related) lacuna, his sole concern with the content of sixteenth-century writings to the exclusion of their form. Of Tahureau's *Dialogues*, Huppert writes, "The more remarkable positions which he stakes out and which I have highlighted are buried in a mass of wordy and pretentious posturing" (p. 32), namely a profusion of classical allusions and quotations. Huppert's way of getting at the gist violates a fundamental principle of historical analysis: namely, that we take the context of an utterance into account, in this case its rhetorical form. Were we to ignore the form of their utterances, we might call Cicero, Horace, and Lucretius philosophes too, for (as Huppert notes) they voiced many ideals cherished by the Enlightenment.

Not only does Huppert ignore the stylistic context of his sources but often the political and religious context as well. Thus the word "Gallican" never appears in his discussion of Pasquier's attitude toward religion, despite the fact that book three of Pasquier's *Researches* is devoted to a consideration of "the ancient liberties of the Gallican church." Huppert is solely concerned with establishing Pasquier's credentials as a philosopher who rejected the stifling hand of religious dogmatism and superstition. But we should note for the record that book three of the *Researches* is devoted to establishing the antiquity and authority of the Gallican church over and against that of the papacy; it is hardly the work of a man seeking to free himself from the heavy hand of religion.

Huppert's single-minded interpretation also reveals itself in his treatment of the story as a struggle between the forces of light and darkness (recall the above-quoted "war against ignorance"). Thus he portrays the so-called philosophes as "good" and their scholastic opponents as "bad." Never does he consider that the rigorously trained scholastics might justifiably have regarded their humanist contemporaries as woolly headed moralists. Of course, one can always choose sides in one's scholarship, but only after exercising the kind of evenhanded historical judgment that takes context fully into account—lest one end up writing Whig history.

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JOHN A. LYNN. *Giant of the Grand Siècle: The French Army, 1610–1715*. New York: Cambridge University Press. 1997. Pp. xx, 651. \$64.95.

John A. Lynn has written a comprehensive history of the French army during the *grand siècle*, 1610–1715. It is a "systematic study" (p. x), but Lynn is the first to admit that it is not definitive. He argues the army was the product of a growing absolutist state, and it ultimately overawed the monarch and government that built it. Financial constraints and quarrelsome nobles, however, constantly hampered the kings' ability to shape their armies, and their appearance, composition, and performance were always less than what was imagined.

More than this, Lynn asserts that despite the volumes written on France in the seventeenth century, historians have largely ignored the army as a key component in state building. He uses the French army to do battle with current trends in historiography, which have consciously sought to dwarf the giant and the field upon which it rests. In structure, the book moves from the general to the specific. To understand rightly the relationship of absolutism, state power, and the army, Lynn believes, one must look beyond topical, thematic, and isolated chronological studies. The reigns of Louis XIII and Louis XIV must be explored together to understand the nature and significance of the army. Evolution, not revolution was the key to French army growth and the institutions that controlled it. In essence, Lynn takes a two-fold approach, arguing that the French army was shaped not only by the personal policies of the French kings and their ministers but also by the general social, economic and political changes in France.

The French created the first large-scale standing army in Europe. It was raised, organized, and directed by the kings and their ministers of war. Its officers and rank and file accepted the notion that it was a royal army: those who served did so at the will of the king. No longer did French monarchs have to rely on military entrepreneurs or state-commissioned armies raised by nobles. Lynn maintains that with a standing army the monarch could do as he pleased regarding foreign policy and state expansion. Therefore, he removed potential checks or threats to his personal power and policies.

The greatest obstacle to building the standing army was that of finances. To fund an army that would reach 400,000 by the Nine Years' War (1688–1697) required economic creativity. Inability to feed and pay the enormous army led to what Lynn refers to as the "tax of violence" (p. 185). Soldiers took what they did not have from their enemies, and often from French peasants and townspeople unfortunate enough to live near the military camps and bivouacs. Seeking to remedy this, the military administration turned to "contributions" levied upon captured towns, cities, and provinces, and lastly, from the financial capacity of their officers (p. 222). "The requirement that colonels

and captains pay a substantial share of the costs of their units out of their own pockets amounted to an unofficial, but recognized tax on the nobility" (p. 223).

With regard to this last form of financial exigency, Lynn makes a very convincing and salient argument. Although the nobility fiercely defended their independence and status as a distinct social class, with all the privileges therein, he asserts that the seventeenth century witnessed an evolution of the French aristocracy. The "culture of command," as Lynn calls it, transformed the nobility's sense of martial honor into "a set of values and practices characteristic of the French officer corps" (p. 248). In essence, the French aristocracy came to see service in the army of the king as an easier means of advancing their station, in contrast to earlier notions of resistance to the monarch as means of exhibiting one's power and privilege.

This desire to maintain independence was seen more at the highest levels of command. Lynn states that powerful nobles such as Louis II, Prince de Condé, better known as the Great Condé, and Henri de la Tour d'Auvergne, Vicomte Turenne exhibited some of the greatest examples of noble defiance toward the growing centralization of the French army. Yet, with the passing of Condé and Turenne, Louis XIV and the Marquis de Louvois, his minister of war, were able to exert better control over their military commanders. Lynn sums up his position, "Turenne disobeyed when he disapproved . . . while Villars complied, even though he might fume" (p. 305). In the quarter century between Turenne and the duke de Villars, therefore, French military leadership came to accept the will of the king as final.

When exploring the composition of the army, Lynn juxtaposes his research to that of French scholar André Corvisier. Corvisier's *L'armée française de la fin du XVII^e siècle au ministère Choiseul: Le soldat* (1964) is the classic work on the early modern French army. Lynn accepts Corvisier's analysis of social composition with some modification but, at the same time, challenges some of his conclusions regarding the motivation of the soldiers. The question raised is whether the armies forged by the kings eventually developed a sense of patriotism. Although Corvisier argues this, Lynn rejects it in favor of a secondary observation made by Corvisier: "that the military experience itself served 'to reinforce national cohesion'" (p. 450).

In such a brief review, it is impossible to do justice to a book of six hundred pages covering one hundred years of an army of 400,000. There is more to Lynn's work than is mentioned here. This is a valuable book, utilizing archival material from the Archives de la guerre, Service historique de l'armée du terre, the Archives nationales, and the Bibliothèque Nationale. Lynn also relies on an extensive array of published works and draws on his previously published articles and books to complete the picture. In short, it is highly recommended for both the scholar and the novice.

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GILLES POSTEL-VINAY, *La terre et l'argent: L'agriculture et le crédit en France du XVIII^e au début du XX^e siècle*. (L'évolution de l'humanité.) Paris: Albin Michel. 1998. Pp. 462. 180 fr.

The literature on rural credit has tended to adopt two basic approaches, the one stressing the peasant's determination to avoid borrowing in order to save and purchase land and the other high levels of peasant indebtedness as a sign of pauperization. Gilles Postel-Vinay, in contrast, insists on the importance of credit as a source of dynamism in a traditional agricultural system adapting to economic change. He reaches his conclusions through the use of economic theory to analyze a significant sample of the notarial archives that provide information on transfers of land and capital. The danger implicit even in a refined econometric approach is the tendency to play down the importance of non-quantifiable factors. Thus, Postel-Vinay prefers to concentrate on the formal credit networks for which relatively standardized records exist. The vast majority of the rural population, however, was largely excluded from these by the demand for security and the costs involved. It remained dependent on intrafamilial and informal transactions, borrowing at rates of interest high enough to offer some protection to the lender against default. Postel-Vinay might have done more to create a more balanced picture. Within these limitations, this is, nevertheless, a brilliant achievement.

The functioning of the traditional credit system depended on the intermediary role of notaries who collected funds from individuals with capital to invest and redistributed it to those who, by reputation, might be expected to be credit worthy. Notaries were uniquely well placed to accumulate information and limit risk and to transfer resources from town to country, from rich to less rich, and from old to young. As a result of fragmented communications and market structures, the geographical scale of their activities was limited. Overall, however, the volume of credit available must have been considerable. The demand for loans often reflected the family life cycle. The costs of establishing the younger generation and of family inheritance transactions loomed large. There was also a widespread, ostentatious desire to (re)build mansions as well as a potentially more productive concern to improve farm buildings, purchase land, and invest in improved farming. The introduction of industrial crops like sugar beets into rotation systems in parts of the Paris region and the north, together with the creation of sugar refineries and alcohol distilleries, substantially increased the need for both working and fixed capital. The supply of credit fluctuated, declining in depressed periods at the very moment when hard-pressed landowners and tenant farmers required "crisis" loans. In general, however, and in spite of widespread poverty, the inequalitarian nature of rural society allowed for savings sufficient to meet demand, at least within the formal capital market.

The cost of borrowing varied according to social status, the importance of the security offered, estimates of risk, and general economic circumstance. As part of the moral economy, and in order to suppress "usury" as well as limit the cost of government borrowing, maximum interest rates of around five percent were set by official decree. The impact was limited. With the exception of periods of crisis and revolution, market rates were generally lower. Credit could be rationed anyway by varying the fees charged, the security demanded, and the period allowed for repayment. When rates were freed between 1789 and 1807, in a period of severe crisis and uncertainty, a major increase was inevitable. The French Revolution had a substantial impact on credit markets. Hyperinflation and the repayment of loans in depreciating *assignats* ruined lenders' confidence for a generation. During the slow reconstitution of notarial credit networks that followed, cautious lenders would, more than ever, force the poorer elements of the population into the informal sector. Subsequent crises, especially those between 1846 and 1851, resulted in further short-term credit restrictions.

An especially intense complex of crises occurred in the last third of the nineteenth century. Increasingly competitive markets and disease affected a wide range of products in some of the most productive regions of French farming and required a rapid reorientation of activity often toward less intensive cultivation. The relatively low cost of loans had encouraged previously what now proved to be an unsustainable level of debt. From the late 1860s, as indebtedness linked to crisis was added to that linked to development, the focus shifted toward debt reduction. Lenders were reluctant to provide capital with the value of the land offered as security declining. This intensified the difficulty of managing the crisis. By the turn of the century, the traditional credit system was in complete disarray. Its decline was gradual, given the absence of viable alternatives. As lenders abandoned the notaries for more secure assurance companies and banks, these were able to offer loans, but they remained highly selective and were unwilling to immobilize capital in long-term loans. The state-subsidized *Crédit agricole* emerged as the major provider of at least short-term credit at relatively low cost. More generally, however, the result of the decline of the traditional credit system was a substantial retreat of capital from agriculture that would last until after World War II.

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PAUL METZNER. *Crescendo of the Virtuoso: Spectacle, Skill, and Self-Promotion in Paris during the Age of Revolution*. (Studies in the History of Society and Culture, number 30.) Berkeley and Los Angeles: University of California Press. 1998. Pp. xii, 385. \$50.00.

Just when the reader starts wondering what connects François André Philidor, Marc Antoine Carême, François Eugène Vidocq, Niccolò Paganini, Franz Liszt, and Jean Eugène Robert-Houdin besides their flamboyant genius, Paul Metzner abandons his biographical approach to situate his subjects "in the context of the general social and cultural characteristics of Paris in the Age of Revolution" (p. 211). His thesis, abbreviated here, is a dense needlepoint of cultural, economic, social, and political threads.

The Age of Revolution, which he brackets from the last quarter of the eighteenth to the first quarter of the nineteenth centuries, saw elite and popular culture flow together after a long separation. At the same time "spectacle-making was encouraged by the proliferation of public spaces . . . the cultivation of technical skills was encouraged by the appreciation in the value of practical knowledge, and . . . self-promotion was encouraged by the dissemination of the self-centered worldview" (p. 2; reiterated pp. 291–92). These concomitant developments were most precocious and intense in Paris. Metzner's virtuosos—the chess genius, the great chef, the criminal turned policeman, the archetypal composer-performers, and the professional magician—made their reputations in Paris; only Paganini and Liszt were not French. The former, along with Robert-Houdin, hid his "secrets." The others proclaimed and publicized theirs.

The great themes of the age—revolution (political, economic, social), the triumph of the bourgeoisie, the solidification of the European state system, and the beginnings of empire—are here, but not causally linked to Metzner's virtuosos. It is the "publicization of social life," as he puts it, along with a growing fascination with machines (and automatons), a love of technical skill in all aspects of life and art, and the cult of the personality that the author stresses. For Metzner, cultural history has its own logic, its own events, its own *longue durée*.

The individual biographies are scintillating, partly because the author has captured the energy, passion, and wizardry of his subjects. The quotations from contemporaries are splendid. They were dazzled by Philidor's ability to play blindfolded, Liszt's sight reading anything put before him, Carême's edible sculptures, and Robert-Houdin's legerdemain; they were willing to believe Paganini had made a pact with the devil in order to play his own *Caprices*. All these prodigies were enacted in public: the amusements and street theater of the boulevards had been raised to an art, carried indoors, legitimated by bourgeois society. Carême was no longer a servant who did the cooking. He was a celebrity for whose services princes and kings bid. Paganini proudly advertised his dubious bestowed patents of nobility. Liszt, younger by almost a generation, was content with the aristocracy of genius. Most amassed fortunes.

Here indeed were careers open to talent, and the virtuosos publicized and propagandized their skills as they climbed to fame and fortune. At the outset, the

lines between spectacle and art, dexterity and creativity were not clear. Even the boy Wolfgang Amadeus Mozart had played with a cloth over his hands and the keyboard. Ludwig van Beethoven had entered piano contests with now forgotten contemporaries. Paganini performed using a cane in place of his bow. Gradually the admixture of charlatanism was abandoned. Robert-Houdin dressed in evening clothes for his presentations. Liszt (although not Paganini) played Beethoven at his concerts. Metzner is impressively erudite. Even the informed and cultivated reader will find new bits of recondite but interesting information, along with the familiar anecdotes.

I have two cavils. Metzner cannot resist playing on words, beginning with the title of his book. More seriously, his central metaphor of virtuoso is made to express all the complexities of European civilization during one of its most turbulent times. The scattered rays of an enormously complex cultural revolution are gathered in the virtuosos. Nearly every important thinker, writer, savant, historical actor, from the eighteenth century to the mid-nineteenth, is invoked and characterized. Napoleon Bonaparte becomes the creator of technocratic government, Jean-Jacques Rousseau the herald of the new genius. Even Immanuel Kant has his part in the creation of a virtuous culture. Metzner's book is itself a kind of virtuoso performance. The Age of Revolution is, if not subordinated, at least most fully expressed in the accomplishments and careers of the virtuosos. Perhaps, but this is an awesome burden for a metaphor.

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DEAN DE LA MOTTE and JEANNENE PRZYBLYSKI, editors. *Making the News: Modernity and the Mass Press in Nineteenth-Century France*. (Studies in Print Culture and the History of the Book.) Amherst: University of Massachusetts Press, 1999. Pp. vii, 386. Cloth \$70.00, paper \$22.95.

This is a broadly thematic collection that summarizes and critiques itself. Editors Dean de la Motte and Jeannene Przyblyski provide an introduction drawing on Richard Terdiman's theories of writing, discourse, and counter-discourse; let ten contributors elaborate case studies; and allow Terdiman himself to have a final, punchy (after)word that compares the acceleration of media information in nineteenth-century France to our own age of the internet and CNN. The work is divided into three categorical imperatives: "The Press and the Politics of Knowledge," "Readers and Consumers," and "Engendering the News."

Jeremy Popkin, probably the dean of nineteenth-century French press studies among American scholars, sets the tone for the volume by arguing for intensely variegated political assumptions and "the transforming possibilities" of the press from the period of the July Monarchy (p. 16). As mere resistance to

censorship is an easy way to make a "political" case, Popkin textures the boulevards, reading rooms, and cafés of public reading "space" to emphasize the ways in which common opposition to the Orleanist regime developed from different social and political projects. Revolutionary politics becomes a "multiplicity of irreconcilable languages," an attack not only on conservatism but also on "the unified public opinion posited by the liberal press" (p. 24). These themes shape the volume.

Following the "multiplicity" approach, Howard G. Lay's rollicking essay on Emile Pouget focuses on Pouget's "wide variety of 'speaks,'" as the anarchist writer sought to speak *to* rather than *for* the people in a calculated colloquial language designed to "provoke laughter, skepticism, and rage" (p. 84). Criminal argots, working-class songs, Communard politics, and farting as civil disobedience become sites of struggle and identity. Identity negotiation is also central to Mary Louise Roberts's and Cheryl Morgan's extended essays on the *Journal des Femmes* (JDF) and the feminist *La Fronde*, whose editors intentionally destabilized boundaries between male and female writing and publishing and linked their journalistic projects to critiques of the gendered social order. Morgan negotiates a space between "women's magazines" as an acceptably domesticated genre of the nineteenth century and the JDF's "pains to define the emancipated, modern woman," a virtual being who "does not exist because she has yet to be formed" (p. 215). Roberts's approach is also that of navigation between worlds, in particular an "ornamental" women's journalism and an "austere" male stylistics. A discussion of "new journalism" is thus a parallel meditation on "the new woman" and the social and political nature of her constitution. Following this, Roberts offers a neatly structured catalogue of nineteenth-century modernity themes: woman/press as prostitute/commodity, reality as "reportage," social life as spectacle, and mimicry as subversion.

This attention to "constructed" narratives thus locates the volume as part of an expanding literature on "modernity" (as the title suggests) and its social, political, and economic institutions. Marketplace and reception issues are examined through essays bearing on commodification of the press, the creation of an information society, and the dynamics of journalistic production and consumption. De la Motte schematizes the links between politics, culture (particularly Romantic), and writing by arguing that "the creation of a new *presse à bon marché* is intimately tied to the so-called social question as it was raised by various utopian socialists" (p. 147). An excursus on Emile de Girardin implicates the development of the mass press with a utilitarian socialist "desire to find unity of purpose" and benefit for the greatest number of people (p. 149).

Maria Adamowicz-Harlasz follows this theme of public accessibility by dissecting the production of the *roman-feuilleton* and its popular readership while

deftly problematizing the “benefit” of attracting wider readerships. As with other pieces in the volume, her work underlines the multiple rather than unitary trajectory of the nineteenth-century press. Attending to the commercial imperatives of the serial novel—in particular, Eugène Sue’s *Le Juif errant* (1844–1845) and the new truncated writing and interrupted character development it required—Adamowicz-Harlasz demonstrates how massification did not create a single public but fueled class and political distinctions between the popular press and the *grands journaux*. While creating a generic consumer, the *roman-feuilleton* fragmented levels of political discussion in the press by blurring the boundaries between commentary and entertainment.

By extension, John Barberet’s essay on Honoré de Balzac completely encapsulates the writer’s empire within commercial imperatives and could be profitably read as a business history. Invoking product placement, market research, and other “modern” strategies of literary production, Barberet details Balzac’s oeuvre as “more than a reconfiguration of the marketing and distribution of books,” but also a studied attempt “to redefine the way people read them, treat them, and feel about them” (p. 194). Barberet’s primary question, which echoes in Terdiman’s conclusions, underlines a very contemporary anxiety: what is the fate of the text in a commercial-capitalist system?

One response finds the editors engaging not only the printed word but also images: the gravure, sketches, and photography, which so characterize popular journalism and shape consciousness of the news. Picking up the theme of censorship, Elizabeth Childs takes a familiar subject—Honoré Daumier’s political cartooning—and does a standout analysis by demonstrating how colonial and orientalist stereotypes of Buddhas, North American “savages,” and Haitian dictators, themselves legitimate targets for ridicule, are simply displaced caricatures of Louis Philippe, Napoleon III, or other “forbidden” subjects. Elaborating the multiplicity theme even more, Przyblyski examines the “montage” of Eugène Appert’s composite photographs featuring Commune women. Here, the events of 1871 are literally created by cut and paste, as the “visible reality” of the photograph combines distinct episodes to invent Commune women as *pétroleuses*, confirming the viewer’s perspective that female deviancy and danger are a priori a form of visible caricature (p. 260).

As a counterpoint to such female representation, James Smith Allen offers up female self-representation by bringing singular attention to the autobiography of Céline Renooz, founder of the Société neosopique and a prolific writer on science and religion. Allen directly addresses Terdiman’s notions of discourse/counter-discourse by positing Renooz’s writing as a deliberate challenge to the dominant operations of French intellectual culture, notably the historical attention paid to the philosophical-religious ideas of Auguste Comte and the historical forgetting of

Renooz. Terdiman’s own conclusion echoes such themes and the volume’s subsuming logic of multiple negotiations with his own emphasis on dominant discourses as, after all, dominant. He maintains a “healthy skepticism concerning claims of radical transformation” (p. 352) and most favorably posits the authors’ mediations as tools of deconstruction, not destruction.

Overall, the editors follow their title as subject, although one might have hoped for more comparative work with other urban constitutencies (i.e. British newspapers) or with news attentive to other interests, including imperial pursuits. Only Childs’s pictures seem to resonate *outré-mer*. The “multiplicities” sought seem internal to Frenchness in a national rather than global way. Still, the volume imaginatively fractures “the press” as it draws us closer to the large modernity and smaller universes of what many French people read—and wrote—as the histories of their own lives. For scholars attentive to, or dependent on, newspapers as document sources, this book will fascinate and challenge.

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DANIEL MOLLENHAUER. *Auf der Suche nach der “wahren Republik”: Die französischen “radicaux” in der frühen Dritten Republik (1870–1890)*. (Pariser historische Studien, number 46.) Bonn: Bouvier. 1997. Pp. viii, 411. DM 150.

The impression left on reviewers who have read this book by Daniel Mollenhauer, a volume in the useful series of works by German historians of France published by the German Historical Institute in Paris, is likely to combine admiration and perplexity.

Anyone interested in the early Third Republic could not but admire Mollenhauer’s diligence and tenacity in beating his way through what he calls the Parisian “library jungle.” His book, a reworking of his doctoral dissertation, reflects a thoroughgoing exploration of primary sources, principally the contemporary press and pamphlet literature, although it is somewhat surprising that the only private papers he found very useful were those of that remarkable individual Alfred Naquet. The author also has full command of the vast secondary literature, both Paris-focused and provincial, both French and “Anglo-Saxon,” bearing on the French far left from the 1870s through the late 1880s, going all the way back to the work of François Goguel (1946) and even Charles Seignobos (1921).

All this research provides a solid foundation for a study of the Radicals of the early Third Republic divided into three principal parts. The first three chapters (comprising part one) describe the growing split, as a result of the vicissitudes of the 1870s, of what had been the Republican bloc of the late Second Empire into Opportunists and “intransigents” still holding out for a hypothetical “real Republic” unlike the bastard set of institutions that had emerged from

the constitution-making compromise of 1875. This is of course a familiar story, and readers acquainted with its outlines need not tarry long over these pages.

Part two, which attempts to discover the essential elements of Radical "identity" as defined by the myth of the "real Republic" that continued to inspire some of them, is more novel and interesting, divided as it is into a variety of topical chapters. For example, a chapter discusses the fidelity of many Radicals to the principle of the imperative mandate, a touching testimony to the faith even politicians once had in the judgment of the common man. Another chapter looks at Radical local electoral committees, which, Mollenhauer argues, need further study, although he does not himself attempt it. Further chapters discuss the Radical campaign for constitutional revision and Radicalism's significantly ambivalent reaction to the resurgence of the social question and to the working-class movements and ideologies that championed collectivist or statist solutions to it.

Part three, truncated by the omission of the dissertation's chapter on the Boulanger episode, is largely a return to narrative, although it argues that the Radical "identity crisis" created by the contrast between their extremist rhetoric and their moderate program helped to generate General Boulanger's movement, which some supported and some opposed, rather than being merely a response to it. Mollenhauer argues, indeed, that while recent scholarship that has deemphasized the left-wing origins of Boulangism is a corrective to older views, it risks "throwing out the baby with the bathwater" (p. 309). The book finds a natural end in the Radical generational turnover of the 1890s, as the men who remembered "how beautiful the Republic was—under the Empire" were replaced by the future managers of the "république des comités."

This solidly researched account, despite the relatively few surprises it holds in store, compels the admiration of a reviewer like me, who published a similar monograph (on a faction of the French right, not left, of the 1870s) using similar sources and similar historiographical assumptions a generation ago. But its appearance in the late 1990s also evokes some perplexity. The author himself notes that his own generation of historians has shown little interest in pre-1900 French political history. Could this be because much of what was worth saying has already been said? What might justify a new book now would be a new interpretive perspective, but this work does not really provide one. It eschews prosopographic investigation of the Radicals, perhaps because comparing the social backgrounds of politicians is nowadays condemned as crude Marxism, although a powerful progenitor of the genre was Sir Lewis Namier, who was no Marxist.

More surprising is the fact that, even as the post-modernist tide of the "new cultural history" is at the flood among young American historians, hardly a drop of it has seeped into Mollenhauer's analytical assumptions. His is a document-based political history that confidently asserts conclusions explicitly based on the

assumption that what his protagonists said and wrote is the key to historical truth. It is solidly in the Rankean tradition. Historians of my generation may be pleasantly surprised to learn that work of this kind can still find a publisher. Younger historians may well dismiss Mollenhauer's evident years of arduous labor as a waste of time, hopelessly misguided both in subject matter and method.

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ARNE JANSSON. *From Swords to Sorrow: Homicide and Suicide in Early Modern Stockholm*. (Stockholm Studies in Economic History, number 30.) Stockholm: Almqvist and Wiksell. 1998. Pp. vii, 197.

Scandinavian historians of the early modern period have done interesting work in fields such as popular culture, crime, and village sociability, the results of which are published mostly in their own languages; hence, an English translation is always welcome. This, despite my criticisms, holds true for Arne Jansson's book (as far as this non-native speaker can judge, the English is not entirely idiomatic). According to Jansson's figures, Stockholm witnessed a declining trend in killing, already manifest by the middle of the seventeenth century. Whereas the homicide rates stood between twenty-five and sixty per 100,000 in the first quarter of that century, by 1700 they were down to five to six. The broad range in the first period is mainly due to a broad range in population estimates. Suicide rates, on the other hand, rose from two to three per 100,000 at the beginning of the seventeenth century to five in the 1710s.

Jansson places his findings in the context of international research on homicide, amply discussing the work of historians and criminologists. It has been established that a long-term decline of homicide occurred in Europe from the later Middle Ages until the 1960s. Most authors explain the long-term trend with reference to Norbert Elias's notion of civilizing processes. Jansson briefly discusses Elias's theories, but he has a narrow understanding of them: he believes there must be an automatic relationship between a civilizing process as a whole (involving changes in the amount and character of self-control) and each constituent element (including a decline in violent impulses). Jansson also discusses the work of social scientists, who, since Émile Durkheim, have investigated suicide. They generally postulate an inverse relationship between homicide and suicide: in regions where the first is high, the second is low, and vice versa. After a careful assessment of the literature, Jansson finds that this principle works most of the time but not always. Historical data, including his own, appear to confirm this negative correlation diachronically. Surveying the social and cultural changes in Stockholm in the seventeenth and eighteenth centuries, the author concludes that a process of individualization has taken place, which, in a Durkheimian fashion, can explain the rise

in suicide. Likewise, it accounted to a great extent for the drop in homicide rates.

According to Jansson, group cohesiveness declined during the period studied, which decreased the motivation to attack people belonging to another group. In particular, in the early decades of the seventeenth century, about half of all homicides resulted from a conflict between a soldier and a civilian, with both groups about equally represented among killers and victims, whereas this was only one fifth around 1700. The obligation to quarter military men without remuneration, which in practice mostly fell on the poorer sectors of the civilian population, greatly fostered animosity between the two groups. In 1688, the authorities abolished this duty, which eliminated a cause of friction between soldiers and civilians.

Was the sharp decline in homicide during the seventeenth century, which Jansson postulates, real? With no body inspection reports preserved, he relies on court records concerning prosecuted cases. Even if complete, this type of source is imperfect for establishing a homicide rate, but even the author admits there are lacunae, while the records of military courts have hardly been preserved at all. Moreover, causing death without intent to kill was increasingly prosecuted as assault rather than manslaughter. The dark number of undetected killings, it appears, rose during the seventeenth century. Although it is unlikely that the number of out-of-court settlements increased, the crucial question concerns fugitives. Jansson thinks that improved policing caused fewer killers to escape the town, but I am not so sure. Since the absolute numbers of homicides fluctuated only slightly, the great drop in the homicide rate between *ca.* 1630 and *ca.* 1650, coincided with a population explosion that was clearly visible despite the range in estimates: the number of inhabitants rose quickly from under 10,000 to over 35,000. It is likely that, in the anonymity of an enlarged Stockholm, more killers managed to flee the town. In that case, the declining trend—I am prepared to accept that there was one—was less abrupt than Jansson claims.

The main interest of this study lies in an intermediate category, that of suicidal murder. Although Jansson finds indications for its occurrence in Denmark and some German territories, it appears to have been especially frequent in Sweden, notably from the 1690s to the 1770s. Some people with a death wish killed in order to be executed: an early modern variant of the contemporary phenomenon of “suicide by cop.” Jansson found sixty-two of such cases: forty in which the perpetrator confirmed the suicidal motive, thirteen probable cases, and nine attempts. Most women committed this form of indirect suicide. They usually slit the throat of a child, not only because it offered less resistance but also because they believed it to be free from sin and hence destined for heaven. These murderers actually wanted to hurt no one, but they had religious objections against direct suicide. Death through execution would save them from eternal dam-

nation. Since this implies an inconsistency in their thoughts, God being ignorant about their intentions, Jansson believes that ancient magical taboos about suicide also were involved. Intriguingly, here we have a crime the prevention of which is obviously served by making it noncapital, but the authorities reacted with the introduction of elaborate pre-execution tortures. Only in the nineteenth century did they substitute imprisonment for the death penalty.

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RISTO ALAPURO, *Suomen synty: Paikallisena ilmiönä 1890–1933* [The Birth of Finland as a Local Phenomenon, 1890–1933]. Helsinki: Hanki Ja Jää. 1995. Pp. 386.

The years preceding and following Finland's independence from Russia in 1917 have been the object of numerous Finnish macrohistorical studies. The parish of Huittinen in Southwest Finland provides Risto Alapuro, author of *State and Revolution in Finland* (1988), with the framework for his microanalysis of the transformation in the complex relationship between mobilization and social group from the 1890s to the 1930s.

Three events structured the Finnish political system and civil society during this time: the general strike and the reform of the Eduskunta (parliament) in 1905–1907; Finnish independence from Russia and the Civil War between the Reds and the Whites that followed in 1917–1919; and the emergence of the extreme rightist Lapua movement in 1930–1932. How did individuals and groups react to these events? How did the left and the right define their relationships? According to Alapuro, these questions can best be elucidated through local analysis that enables us to see how people got together, how they defined their group and its relations with other groups, and how they became conscious of their class membership and nationality.

Since the second half of the nineteenth century, primary education and voluntary associations had spread the idea of a common citizenship and cultural equality. When Fenno-Russian relations were severed in 1899 as a result of Russian attempts to curb sentiments of independence, grass-roots organizations of various kinds proliferated. While the Finnish general strike of 1905 was a reprise of the Russian general strike, it was also a sign of liberation from Russian tutelage and an occasion to unite the democratic forces.

At the end of the nineteenth century, there was no longer any nobility or bourgeoisie in Huittinen. In 1901, eighty percent of the inhabitants of the parish got their daily bread from agriculture. Class opposition took various forms. At the top of the social hierarchy were the civil servants and the large landowners, the “peasant nobility,” whereas the majority of the population consisted of crofters and various agricultural

workers. A cleavage also existed between factory workers and factory owners and managers. The middle class formed a group apart.

In 1898, the large landowners created the Huittinen cooperative dairy, which soon attracted most of the area's agricultural producers, including the poorer farmers. All social groups participated in the founding of Huittinen's cooperative store in 1904. As a result, the crofters were drawn into a market economy that also created common interests with the larger landowners; in the 1920s and 1930s, both would approve the ideology of the free peasantry, and in the 1930s, some of them would support the Lapua movement. The founding of the People's High School of Southwest Finland in 1892 provided a forum through which the "idealist-patriotic spirit" (p. 59) could be spread to the larger population. With some civil servants and intellectuals, manual workers created Huittinen's volunteer fire brigade and a worker's association. Apart from an ephemeral temperance organization, founded in 1886, these were Huittinen's first workers' organizations. Through this mobilization, class and nation were introduced into the local context.

Initially, the inhabitants of Huittinen received the news of the Russian Revolution of February 1917 with patriotic enthusiasm. Attracting "without discrimination people from all social strata" (p. 157), the first public meeting took place at the headquarters of the volunteer fire brigade. Because of the general insecurity caused by the February Revolution, both left and right created their own armed security forces, the Red Guard and the Civil Guard. The revolution arrived in Huittinen on February 4, 1918, when the Red Guards took control of the parish. With the revolution came its first casualty, Frans Joel Malmberg, a baker and the son of a crofter. In the maelstrom that followed Malmberg's murder, the Red Guards accidentally shot one of their own. According to Alapuro, however, the local workers' revolution was not based on hatred of the existing order or the state. Their leaders were dragged into the struggle to defend the workers against the local bourgeoisie, while the winners, the Whites, were freeing Finland from Russia.

In this fine, well-documented study, Alapuro convincingly demonstrates that Finland was an exception to the rule that "the voting booth is the coffin of revolutionary movements" (p. 307). Here, democracy preceded revolution and not vice versa. Through the study of one locality, Huittinen, Alapuro sheds light on the complex psychological and social obstacles that a whole nation had to overcome.

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PETER R. PRIESTER, *Geschiedenis van de Zeeuwse landbouw circa 1600–1910*. (A. A. G. Bijdragen, number 37; HES Studia Historia, number 20.) Goy-Houten, the Netherlands: HES Uitgevers, 1998. Pp. 880.

This authoritative history of agriculture in the Dutch province of Zeeland from the beginning of the seventeenth to the early twentieth centuries has two main themes: the nature of the dominant agricultural system, which had already been established by the beginning of this period, and the reason farming in Zeeland underwent relatively little change before the second half of the nineteenth century.

Zeeland differed from the other maritime provinces of the Netherlands during the greater part of this period because of its concentration on arable—with wheat as the most important crop—rather than pastoral and dairy farming. Peter R. Priester distinguishes three main phases in the agricultural history of the province during these three centuries: from 1600 to around 1670, a rapid expansion of the cultivated area through reclamations; from 1670 to 1870, large-scale production of madder, especially in the north of the province; and from 1870 onward, new technology, new crops (notably sugar beets), and changed economic circumstances finally brought an end to a system of agriculture that dated back to the late sixteenth century at least.

By the nineteenth century, Zeeland farmers were coming under criticism from reform-minded contemporaries for what was seen as stubborn resistance to change of any sort, and historians have in the main echoed these criticisms. Priester takes issue with this established view and argues that farming in the province had already reached a high level of productivity by the beginning of the seventeenth century, and it was this relative efficiency of Zeeland's agriculture, rather than any blind conservatism on the part of the farmers, that explains the lack of fundamental change before the late nineteenth century. The large-scale introduction of madder in the late seventeenth century as a response to falling grain prices shows that farmers were willing to make quite drastic changes when the need and opportunity arose. There was no agricultural revolution in this period—only because it had effectively already taken place in the late Middle Ages.

This study is a good example of the new agricultural history and attempts to strike a balance between description of farming techniques and a broader economic approach. Priester's sense of relevant context is admirably broad, including not only ecological but also fiscal, political, and social factors. Much of this context comes from original research: for example, in the chapter on demography, Priester includes what is now the best available survey of Zeeland's population history for the seventeenth and eighteenth centuries. Geographical and ecological conditions were, of course, of fundamental importance and had very distinctive characteristics in this region. The increase in the area of land available for farming through drainage and reclamation from the sea was one of the most important factors. There was particularly rapid growth in the first half of the seventeenth century, when the area of land under cultivation in Zeeland Flanders (now known as Zeeland below the Westerschelde)

more than tripled, and in general the rate of reclamation varied throughout the period according to the price of agricultural products as well as hydrological circumstances. Variations in soil quality were also marked, particularly the difference between the fertility of new polders and older land. Priester argues that a crucial limitation on the possibilities open to farmers on the Zeeland islands was the shortage of fresh water, as this severely restricted the number of cattle that could be supported. The small number of cattle, in turn, limited the supply of animal fertilizer. Endemic malaria and the cost of maintaining the defenses against the sea were further burdens on the agriculture of the province. The introduction of new technologies, new plant strains, and, in particular, artificial fertilizer in the later nineteenth century made it possible to break out of many of these constraints, and then agriculture in the province was transformed within a few decades.

This is an important contribution to the history of Dutch agriculture in its economic, social, and ecological context. A particularly valuable aspect of the book is a set of appendixes running to over 300 pages; these provide a substantial part of the data on which the analysis of the first half is based, giving it something of the character of a source publication. In many ways, this exemplary study illuminates the social as much as the economic history of the province through three centuries. However, the author's decision to take 1600 as a starting point leaves unanswered the question of when and how this remarkably successful farming system first developed.

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ANDREAS EDEL. *Der Kaiser und Kurpfalz: Eine Studie zu den Grundelementen politischen Handelns bei Maximilian II. (1564–1576)*. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 58.). Göttingen: Vandenhoeck und Ruprecht. 1997. Pp. 520.

As the author of this copiously documented monograph notes, it has been the ambiguous confessional leanings of Emperor Maximilian II (r. 1564–1576) that have excited historical imaginations. The political Maximilian, even Maximilian the religious politician, has not received the same scholarly attention. Andreas Edel proposes to fill this gap, although he gives no explicit reason for doing so. Edel does announce, however, that his work has been shaped by models of decision-making processes found in political science. Nevertheless, he almost immediately backs away from rigid adherence to such paradigms with the observation that historical data inevitably qualifies the clinical abstractions of the social sciences.

Edel's book, therefore, turns out to be an informative, albeit rambling, monograph on imperial politics rather than the methodological experiment promised in the introduction. Proceeding as a topical research

report, it often ranges substantially beyond the limits of its title. Individual sections are valuable, both for their content and for Edel's analysis, but the general impression of the work is that it frequently spills over the organizational structure that the author hoped to give it.

Edel opens with a thoughtful and concise essay on the fluid constitutional structure of the Germany within which Maximilian worked. His examination of the operational subtleties of the 1555 Peace of Augsburg is especially good, as is his summary of the complicated political relationships that Maximilian developed with the German princes on confessional policies. The second section is a quasi-biographical account of Maximilian II's personal development, which is again very insightful, indeed to be recommended to anyone looking for a quick sense of the man and his career. Nevertheless, even here, Edel stuffs his account way beyond the needs of his inquiry. The detailed account of Maximilian's deathbed dramatics contributes little to an understanding of his political decision making. For all of the originality of some of Edel's material in this part of his text—his treatment of Maximilian's intelligence network, for example—his final conclusions about the emperor largely echo those of others (J. H. Elliot's description of the emperor as an "honest broker" in German confessional affairs, for example). But Edel understands Maximilian well and explodes some misperceptions of him, too. The emperor, for all his talk of peace, was hardly a pacifist. On the vexed question of Maximilian's religious convictions, Edel advises us to take the man more or less at his word. Failing new archival finds, we may very well have to do that.

The bulk of the book examines the complicated and often hostile relationship of Maximilian and Elector Palatine Frederick III, who introduced Calvinism into his lands in the early 1560s. Much of this question, especially the crucial Diet of Augsburg on 1566, in which Frederick faced down the emperor's pressure to eradicate Calvinism in the empire, has been heavily researched, as Edel himself acknowledges (p. 190). Nevertheless, his presentation of this and other major imperial conclaves contains interesting fresh material on the political concerns of the German imperial princes, which the author, who has a sharp sense of the men he is dealing with, uses effectively to enliven his text. One should be aware, however, that Edel plows a path with many byways while setting out his findings. A section on Maximilian's feeble attempts to control his public image, which follows a good introduction to the confessional landscape of the Rhenish holdings of the Wittelsbachs, seems conspicuously misplaced.

It may be perverse to find faults with the scrupulous research Edel has done. Nevertheless, there are some lapses. Although these do not seriously detract from the study's central thrust, they could mislead any scholar who uses the book selectively. Maximilian's wife, his Spanish cousin Maria, had a far greater grasp of practical politics than Edel would allow. Helga

Widorn's dissertation, *Die spanische Gemahlinnen der Kaiser Maximilian II., Ferdinand III. und Leopold I.* (1959), which does not appear in an otherwise admirable bibliography, might have made Edel a bit more cautious here. Where he has done little research, such as on Maximilian's economic policies (p. 135), Edel falls back on rather speculative associations with Erasmus political philosophy to explain the emperor's idea of the "Common Good" (*der gemeine Nutz*). Indeed, Maximilian had a rather practical notion of what the expression meant, but one has to look directly at some of his comments about financial administration in order to learn this. Edel's account of Maximilian's religious development is incomplete without some consideration of the Italian spiritual writer, Jacopo Acontius, whose ideas closely paralleled Maximilian's and whose work the emperor apparently knew. The otherwise sound explanation of Maximilian's humiliating failure to stamp out Calvinism in Germany in 1566 is somewhat weakened by Edel's neglect of the role that Maximilian's association with the papal nuncio at the gathering, Giovanni Giacomo Commendone, played in making the emperor suspect in all Protestant eyes.

Edel's immense collection of data contextualizes Maximilian's religious policies in Germany, with special reference to the Palatinate, persuasively, even provocatively. In a work that seems to incorporate almost every piece of data that crossed the author's line of sight, however, the parameters of Maximilian's religious politics are not really fixed. If anything, Edel appears to have found the elasticity of an infinitely expanding universe far more congenial to his task.

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HANS MEDICK. *Weben und Überleben in Laichingen 1650–1900: Lokalgeschichte als allgemeine Geschichte.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 126.) Göttingen: Vandenhoeck und Ruprecht, 1996. Pp. 708.

In 1977, Hans Medick, in collaboration with Peter Kriedte and Jürgen Schlumbohm, published *Industrialisierung vor der Industrialisierung*, a theoretical analysis of the ties between rural industry in early modern Europe and the genesis of capitalism. The authors regarded this controversial work as a prolegomenon to the ambitious local studies of proto-industrialization that each had just started. Since then, Medick has devoted much of his scholarly energies to recovering the social, economic, and demographic history of Laichingen, a small Swabian village that in the eighteenth century became the principal center of rural linen production in the duchy of Württemberg. Not surprisingly, his extraordinary study of this community represents in large measure an elaborate empirical test of arguments that he and his collaborators proposed two decades ago. In many respects, however, Laichin-

gen did not resemble the typical rural or even proto-industrial community. Medick deftly exploits this seemingly unfavorable circumstance. By thematizing Laichingen's exceptionalism, he offers fresh insights into fundamentally important issues concerning the economy and society of pre-industrial Central Europe.

One supposed instance of this exceptionalism stands in doubt, however. Before superbly analyzing the growth of Laichingen's linen industry and the efforts of its weavers, who belonged to a regional guild, and others to sell cloth in violation of the monopolistic privileges held by the merchant company in the town of Urach, Medick claims that proto-industry in Central Europe generally flourished where guilds did not exist. But in a major revision of the previous scholarship on this matter, Sheilagh Ogilvie (*State Corporatism and Proto-Industry: The Württemberg Black Forest, 1580–1797* [1997]) convincingly argues that many proto-industrial regions had occupational corporations. Thus the institutional and political environment in which linen manufacturing in Laichingen developed was probably much more typical than Medick cares to admit.

Part of Laichingen's exceptionalism reflected a broader regional pattern: the extensive commingling of agricultural and artisanal employment in southwest Germany's countryside. Medick explores how this pattern related to the changing fortunes of proto-industrial households and contributed to the distinctiveness of Württemberg's industrialization in the nineteenth and twentieth centuries. He also traces the innovations, changes in the production process, and new marketing practices by which the linen proto-industry survived in Laichingen during the nineteenth century, whereas it shriveled elsewhere in Württemberg because of unfavorable international market conditions. Moreover, throughout his extended analysis, Medick skillfully uses tax returns and inventories after death to reconstruct credit relations, the distribution of wealth, and patterns of wealth mobility, thereby revealing the social groups that either gained or lost because of proto-industrialization.

Exceptionalism also characterized Laichingen's demographic regime. Using family reconstitution data gathered from 2,594 households (an extremely large sample for a community with roughly 1,500 inhabitants during most of the eighteenth and early nineteenth centuries and only 2,700 inhabitants in 1880), Medick provides an extraordinarily detailed analysis of fertility, mortality, and nuptiality (including intergenerational wealth transfers at the time of marriage). Thus he shows that proto-industry did not affect the age at marriage and marital fertility rates in Laichingen as it did in many other regions. More remarkably, Laichingen's extremely high marital fertility rates (higher than the Hutterites') scarcely offset its appallingly high infant mortality rates, which rose during the nineteenth century because of an intensification of female agricultural labor. In interpreting these findings and those from other studies, Medick argues that early

modern Germany had two demographic systems. The one in the North featured low marital fertility and infant mortality, while the one in the South and Southwest resembled Laichingen's. Moreover, these two systems stemmed largely from regional differences in weaning and breast-feeding practices. Medick's argument, which complements that recently made by Schlumbohm and builds upon earlier work by John Knodel, has considerable merit and should influence future research. Given Medick's interest in determining proto-industrialization's effects on the growth of Laichingen's population, however, why does he completely ignore remarriage? Through this institution, a population recovered some of the fertility lost because of a marriage partner's early death.

Using almost 1,500 marriage inventories and inventories after death, Medick presents an exemplary analysis of the books and clothing owned in eighteenth and early nineteenth-century Laichingen. The results show the social circuitry by which Enlightenment culture infiltrated village life. While methodologically similar to Daniel Roche's pioneering work for France, Medick's treatment of sartorial tastes rests on a significantly better selection of sources. Reading practices introduce another facet of Laichingen's exceptionalism, for the rates of book ownership in this rural community exceeded those in Tübingen and other cities. Almost exclusively religious in nature, these books yield valuable clues about how men's devotional concerns and sensibilities differed from women's and about the reception of Pietism in the late eighteenth century. Medick could have contextualized his findings more, however. His focus on religious books unavoidably creates the impression of a deeply pious proto-industrial society. But other eighteenth-century sources—especially consistory records—from southwest Germany indicate a pervasive moral and religious laxity in the countryside. Do not Laichingen's rich sources shed some light on topics such as profanity, sexual misconduct, and observance of the Sabbath?

Finally, Medick's splendid book treats methodology lucidly, thoroughly, and incisively. The discussions range from the frank acknowledgment of incorrect assumptions in his earlier speculations on proto-industrialization to critical assessments of the strengths and limitations of the local records, including a notorious forgery. Scholars interested in the technique of micro-history will find enormous richness in this work.

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MICHAELA HOHKAMP, *Herrschaft in der Herrschaft: Die vorderösterreichische Obervogtei Triberg von 1737 bis 1780*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, vol. 142.) Göttingen: Vandenhoeck und Ruprecht, 1998. Pp. 283. DM 62.

Eighteenth-century absolutism rarely lived up to its ambitions, but it would be mistaken to deduce from

this the failure of effective rule, even in districts remote from the centers of power. In this carefully argued and well-written microhistorical study of power relations in the Black Forest lordship of Triberg, Michaela Hohkamp shows how practices that indicated sloppy administration from the perspective of capital cities reflected the indispensable compromises upon which effective and stable rule depended locally. Triberg was doubly peripheral: this mountainous lordship was off the beaten path even within the remote Habsburg exclave of Vorderösterreich, with its provincial seat in the town of Freiburg im Breisgau. By virtue of its marginality, Triberg is well suited to illustrate Hohkamp's main points: that no history of early modern governance is complete that fails to consider the effects of its embeddedness in a creative interplay of local forces, and therefore that binary conceptual oppositions—resistance versus obedience, community versus authority—are inadequate to convey its full complexity. Rather, Hohkamp shows how royal intentions, peasant behaviors, and the maneuvering room available to local governors were constantly shaped and remade by interaction among all parties to the process. This, then, is a history of power written neither from the "top down" nor the "bottom up," but from the "outside in."

Hohkamp pursues her subject on two tracks. The first three chapters are devoted to the administrative practice of Franz Meinrad von Plummern and his son, Franz Joseph. Triberg's royal governors (*Obervögte* or High Stewards) during the reign of Maria Theresia. The goal of these chapters is to reveal the constraints on central decision making: the governors' dependence on local sources of income and authority, for example, forced them constantly to balance royal desires against the peasants' expectations. This often led the Plummerns to thwart centralizing reforms—such as an attempt in 1767–68 to introduce guild restrictions on the production of clocks—in the interest of preserving stability. The result was a cooperative style of governance based on integrating subjects' wishes and needs, an approach the peasantry knew how to manipulate. A similar combination of dependencies reproduced analogous patterns of exchange in each village, where overseers, foresters, and tariff officers balanced the governors' orders against needs and expectations of local elites. Village constables (*Hatschiere*) and other office holders, for example, denounced misdeeds with a social bias against land-poor subjects that indicates their subservience to land-owning peasants. Not surprisingly, the latter were the decisive social group in these negotiations.

These relationships were anything but static. In chapters four and five, Hohkamp analyzes the governors' daily court protocols to reveal how Triberg's women and men made use of judicial institutions. These data allow her to reconstruct an evolving relationship between proto-industry, property relations, honor, and gender. The 1750s, it turns out, were a crucial turning point: Hohkamp's study of inheritance

disputes shows, for example, that by 1750 women had been virtually excluded from inheriting farms and had almost disappeared from the ranks of litigants. Similarly, the vast majority of disputes over private debt—seventy percent—arose before 1750. The Tribergers had always tended to keep domestic disputes out of court, but three quarters of conflicts over honor arose after mid-century. Why the change? Hohkamp suggests that disputes over debt declined because the spread of rural clockmaking relieved subsistence pressure on the propertyless classes. Consequently, the battleground of social differentiation shifted from landowning to the terrain of honor, which sharpened and gendered the divide between public and private. More and more, the honor of married males depended on their reputation as good housefathers and defenders of women under their authority, an ideal that was inconsistent with independent self-defense by women. As women retreated from public view, men advanced. The social ideal of the good housefather, in turn, encouraged the use of courts as an alternative to brawling. Increasingly, the Tribergers transferred their disputes from one public space (the tavern) to another (the courthouse), with stabilizing effects on local governance generally. Socially, this shift reflected the efforts of Triberg's landowners to retain the upper hand through an alliance with the governors von Pflummern. But on balance, Hohkamp concludes, the decision to litigate was determined first by gender and only secondarily by social condition or civil status.

Hohkamp's delicate interweaving of social, cultural, and political forces indicates the enriching possibilities of microhistorical approaches to histories of power and gender. One wonders how much her findings apply elsewhere; might her description of grass-roots bureaucratization of social conflicts contain a new explanation for the decline of peasant unrest in early modern Germany? Wisely, Hohkamp hesitates to generalize from her admittedly narrow base. But this does not detract from the great value this book has for students of early modern rural society and politics and for all historians of gender, crime, and honor.

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JAMES M. BROPHY, *Capitalism, Politics, and Railroads in Prussia, 1830–1870*. (Historical Perspectives on Business Enterprise.) Columbus: Ohio State University Press, 1998. Pp. xi, 273. \$50.00.

German historians, like their American counterparts, have been chipping away at long-held images of nineteenth-century state-economy relations. Where American historians once privileged society over state, German historians did the opposite, almost reflexively placing the state at center stage. In recent years, however, as new research has uncovered an often powerful American state, scholars have tempered their view of the Prusso-German state's power. For German

historians, this has encouraged a fresh look at agency in the private sector.

James M. Brophy's book takes this transformation in German historiography to a new level. Exploring relations between railroad entrepreneurs and Prussian state officials between 1830 and 1870, Brophy offers a richly detailed study of "the business class" and its complex relations with the state. Like Jeffry M. Diefendorf and Hartmut Kaelble, Brophy focuses on businessmen in action, but where they studied a particular locale (the Rhineland and Berlin, respectively), he confines his attention to a single sector, then ranges broadly across the Prussian provinces.

Brophy reaches similar conclusions but with a wider reach. Like Diefendorf and Kaelble, he argues that the notion of an "alliance" between nobility and bourgeoisie, long dominant in the literature on mid-century Prussia, does not capture the complexity of state-economy relations. Above all, he finds, Prussian railroad men were consistently pragmatic. They were as capable of effective opposition when state policies threatened their interests as they were of cooperation when it suited them. Which posture they adopted depended on the issue as well as on prevailing business conditions. The study's geographic scope and the diversity of the railroad men's economic activities give reason to think that this "search for mutual accommodation" (p. 20) characterized state-economy relations generally in Prussia during these decades.

Brophy builds his argument in chapters that move chronologically and topically, based on a wealth of archival research. Chapter two explores "the railroad question" from 1830 to 1848: that is, the critical question of whether the state or private capital would build railroads. Prussian officials ultimately left the task largely to private capital, as others have shown, and Brophy's account does not alter that understanding, although he adds nuance and detail. His contribution comes in the remaining chapters, which offer valuable insights into the understudied years from 1848 to 1870. Two chapters devoted to state-railway relations during business cycles in 1848–1857 and 1857–1870 bracket topical chapters on state efforts to force the railroads to run night trains, on conflict over banking policy, and on struggles to control state funds set aside to promote railroad development.

In these chapters, Brophy explores not only tension between railroad promoters and state officials but also, building on Eric Dorn Brose's work, deep-seated conflict among state officials themselves. State-railway tensions lay squarely at the center of the conflict over night trains, for example. Beginning in 1849, Minister of Trade, Commerce, and Public Works August von der Heydt, who fought tenaciously to bring the private railroads under state control after 1848, ordered them to run night trains. Although he ultimately succeeded, the railroads mounted formidable resistance, challenging his order in court and in a few cases refusing to comply even after the state took over management. In conflicts over banking policy and the Railroad Fund,

however, more complicated lines of conflict emerged. Heydt proved single-minded in his pursuit of state railroads, while his fellow ministers often differed sharply with him and among themselves on policy issues as well as political strategy.

Prussian railroad men made good use of the opportunities created by these divisions in the state. Persistently pursuing their interests, they employed all available tools—parliament, the press, business associations, and personal contacts—to shape state policies. By the early 1860s, Brophy argues, they had achieved a “juste milieu,” that is, “a social-political atmosphere in which entrepreneurial elites saw their interests nurtured by the state” (p. 135). Consequently, they had little reason to pursue radical changes in the Prussian state. Their own political success, Brophy concludes, helped to forge the structural continuities that marked postrevolutionary German history.

Although Brophy does substantial damage to the conventional image of state-economy relations, he could have pushed further. He steadfastly views the railroad men as representative of “the business class,” even though his own evidence hints at conflicts among business interests that surely deepened the complexity of state-economy relations. Drawing on secondary sources to flesh out the comparative context, moreover, he could have moved further beyond the conventional stereotypes. Prussian state policies that he characterizes as “paternalistic”—offering financial assistance, passing favorable legislation (p. 107)—resembled those of states (even the American) not conventionally regarded as paternalistic. But all that said, Brophy succeeds admirably in establishing new respect for the political success of mid-century Prussian businessmen.

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JAMES H. JACKSON, JR. *Migration and Urbanization in the Ruhr Valley 1821–1914*. (Studies in Central European Histories.) Atlantic Highlands, N.J.: Humanities Press International. 1997. Pp. xix, 452. \$85.00.

James H. Jackson has documented migration to and from Duisburg during the great age of urban growth with extraordinary thoroughness. Making use of unusually detailed sources in archives both there and elsewhere in the federal German state of North Rhine-Westphalia, he has painted a very detailed picture of migratory behavior into and out of a town that grew, as a result of heavy industrialization, from a population of 5,594 in 1805 to one of almost 230,000 in 1910. Presenting his results not only in clear prose but also in 145 tables and graphs and in nine maps, Jackson has produced a work that can lay claim not only to definitiveness as a study of Duisburg but also to singularity as a study of rural to urban (and urban to rural) migration anywhere.

Although some readers may be put off by the

quantitative apparatus (and that part of the methodological apparatus that is contained in lengthy reference notes and in seventy-five pages of appendixes), they should not be. Jackson's work is not animated simply by a desire to amass data. He deploys his findings argumentatively, for the purpose of advancing a nuanced but basically clear view of the migratory process. It would be an exaggeration to say that this view is revolutionary; others have espoused it. But no one has done so—to this reviewer's knowledge—as systematically as has Jackson.

Jackson takes as his conceptual point of departure a belief, which was particularly prominent among German commentators around 1900, in the close links between urbanization on the one hand and disorganization, anomie, and deviant behavior on the other. While continually referring to the ideas of men such as Ferdinand Tönnies, Georg Simmel, and the many less well known but even more forceful critics of urban society who wrote contemporaneously with many of the developments he describes, Jackson continually seeks to undercut them. In his view, the lives of the men and women who lived in and around Duisburg displayed a far higher degree of stability and coherence than the classic critiques lead one to expect.

Jackson supports his argument effectively in several ways. He shows that the overwhelming majority of migrants formed households. The typical newcomer—a young, single, male worker—settled down, married, and, with his wife, produced children at a rate that exceeded the rate for Germany as a whole. Although rates of both property crime and violent crime also exceeded those in the rest of the country, there was no clear link between urbanization and rates of suicide. Moreover, migrants did not become cut off from their roots outside the city. Many of them engaged in circular patterns of movement, returning after stays of varying lengths in the city to their places of origin in smaller towns and in the countryside.

The book unfortunately lacks comparisons between Duisburg and other cities. This omission is most serious with regard to birthrates and the natural increase of population. Given the fact that big cities in general would not have grown (at least not very much) without a migratory surplus, one would like to know why Duisburg seems to have differed from the general pattern. But because the comparison is not made—at any rate, not systematically—the question is not raised. One would also like, if only in the book's conclusion, to see some indications of ways in which Duisburg's experience was typical. The force of the book should lie—and implicitly does lie—not in what was peculiar about one city but in the ways in which its history illuminates the cityscape more generally, but the reader could use some more help in this regard.

It also bears noting, with regard to Duisburg itself, that Jackson achieves his quantitative exhaustiveness in the areas of migration and other aspects of demographic history at the expense of treating many other areas of the city's life. Although a dozen photographs

give one a good feel for the look of the city, there is little more than passing attention to the city's institutional history. Nowhere do we learn much about such phenomena as the private associations, the religious communities, or the governmental agencies in which—even if most migrants were not directly involved—efforts were presumably being made to facilitate their adaptation to urban life. I make this observation not as a criticism but by way of pointing out the nature of the limitations Jackson had to impose upon himself in order, after truly Herculean labors, to bring his precise and thorough study to a successful conclusion.

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HANS-PETER GOLDBERG. *Bismarck und seine Gegner: Die politische Rhetorik im kaiserlichen Reichstag*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 112.) Düsseldorf: Droste. 1998. Pp. 559. DM 110.28.

After agreeing to do this review, I will admit to having had second thoughts about the historical interest or relevance of a study of political rhetoric in Bismarckian Germany. The book's introduction, in which Hans-Peter Goldberg proclaims his intention of applying Aristotelian and Ciceronian theories of rhetoric to speeches given in the Reichstag in the decades before 1900, only increased my doubts. Following such a theme for over 500 closely printed pages—in what the author admitted was a very abbreviated version of the original plans for his dissertation research—seemed a less than enthralling enterprise.

On going ahead, however, I was very pleasantly surprised. Avoiding obscure Latin terminology, Goldberg's account of parliamentary oratory was clear and elegant, at times even intriguing. His study of rhetoric, far from being an arid and ahistorical application of formal categories, involved elucidating and demonstrating the uses of different rhetorical forms in the context of the political struggles and institutional surroundings of the early decades of the German Empire. The upshot is an impressive interdisciplinary work that builds on both previous historical and rhetorical scholarship, offers very keen insights into the biographies of the speakers analyzed, and is an excellent contribution to the study of German political culture in the last third of the nineteenth century. This brief review can only mention in summary form a very few of the book's many positive features.

Goldberg's study begins with an illuminating account of the physical and institutional circumstances of parliamentary debate: the architecture of the temporary structure housing the Reichstag in its first quarter century, the relationship of parliamentary speakers to their party caucuses, the press and the galleries, and the customs and practices of parliamentary procedure in Bismarckian Germany. Then he moves to the heart of his investigation, the analysis of the rhetorical styles

of four major parliamentary speakers: Bismarck himself and his three most important parliamentary opponents, the Social Democrat August Bebel, the left-liberal Eugen Richter, and the (Catholic) Center Party leader Ludwig Windthorst. Each speaker receives a lengthy chapter containing a wealth of interesting examples, illustrated by clever commentary. The author discusses Bebel's rational and analytical speeches, Richter's acid, sarcastic humor, and Windthorst's rambling, inconclusive speeches, and their wit and self-deprecating irony, and Bismarck's "anti-rhetorical rhetoric." In each instance, Goldberg links the rhetoric not only to the biographies and personalities of the individual speakers but also to their political positions and dilemmas. I found his section on Richter most insightful and rewarding (perhaps because he is the least well studied of the four figures), but there was always something new to learn in Goldberg's accounts of the other three.

Rhetoricians may or may not be concerned with the reception and effect of speeches, but historians certainly are. Although the author provides a good deal of material on this question, discussing, for instance, parliamentarians' responses to speeches (catcalls, laughter, applause) recorded in the stenographic record of the Reichstag debates, or considering the role of journalism in bringing these parliamentary debates to a broader public, Goldberg ultimately dodges a definitive answer or even a clear conceptualization. His study of oratory considers its simultaneous involvement in two different discursive universes: one in which the addressees are fellow parliamentarians, and another in which parliament is used as a tribune to address a wider audience. Goldberg can show examples of each, such as Richter's invention of the German evening newspaper, largely as a way to get his parliamentary speeches to a broader public, or Windthorst's repertoire of inside humor, which reached the point that the Catholic parliamentarian only had to mention university professors (a favorite butt of his jokes) for everyone in the Reichstag to start laughing.

This distinction is an important one, because the book is a study of the Bismarckian era as a high point of parliamentary oratory. The author notes the widespread public perception, both by contemporaries and later historians, of Wilhelmine parliamentarians as inferior and uninteresting speakers. He also briefly compares the gentlemanly and collegial rhetorical practices of the German Empire with the savagely polemical and often hate-filled speeches in the Reichstag of the Weimar Republic or the increasingly technical parliamentary addresses of the post-1945 Federal Republic, which often lacked any apparent audience. Such a periodization implicitly involves an evaluation of the reception and impact of political rhetoric but the author's concluding account of the reasons for this periodization is unconvincing, and an explicit consideration of the different universes of parliamentary discourse might have helped. This problem aside, I strongly recommend this book to anyone interested in

the political culture of the nineteenth century. Readers not intimidated by the book's length, its technical introduction, or its seemingly forbidding topic will be rewarded with a rich and intriguing work.

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HEIKE FRANZ. *Zwischen Markt und Profession: Betriebswirte in Deutschland im Spannungsfeld von Bildungs- und Wirtschaftsbürgertum (1900–1945)*. (Bürgertum: Beiträge zur europäischen Gesellschaftsgeschichte, number 11.) Göttingen: Vandenhoeck und Ruprecht. 1998. Pp. vii. 341. DM 94.

Heike Franz's monograph was originally a dissertation completed under the direction of Peter Lundgreen and Klaus Tenfelde, experts in the history of the German educated middle classes (*Bildungsbürgertum*) in the nineteenth and twentieth centuries, respectively. It is the purpose of Franz's book to describe the "formation of business administrators as a new academic professional group . . . between 1898 and 1945" (p. 1). Franz rightly claims that, except for the work of Robert Locke and David Lindenfeld, little attention has been paid to the professional history of business administration in Germany. Franz is wrong to claim, however, that the "history of the academic professions in the twentieth century has as a whole hardly been researched" (pp. 2–3). This claim is contradicted by her own appropriate citation of the large extant literature in German and English, primarily from the 1980s and early 1990s, on just this subject, including Charles McClelland's *The German Experience of Professionalization* (1991), which is referred to in both text and notes but then difficult to find in the bibliography as it is entered between "Chandler" and "Cisk." It is also a dubious claim of the author that the relationship between the history of professionalization and the wider history of the bourgeoisie in Germany and Europe has not heretofore been explored in the literature. A look, for example, at Konrad Jarausch's *The Unfree Professions: German Lawyers, Teachers, and Engineers 1900–1950* (1990) is enough to confirm that work along such lines is anything but unprecedented.

Franz's book, however, is a welcome addition to the literature on the professions and the educated middle classes in Germany, especially through its relatively unique—though still also not unprecedented—emphasis on the twentieth century instead of the nineteenth. Franz locates her research methodologically as a balance between earlier ideal-type and functional analyses and the more recent "conflict-historical" (p. 3) literature, an approach that explores both the academic institutionalization of the field of business administration and its organized—and failed—strategies toward the goal of market monopolization. Franz stresses the importance of not being bound simply to a functional-institutional framework for understanding the history of professions. Following the lead of Chris-

tina von Hodenberg, she seeks to write a cultural history that includes consideration of "collective socialization . . . norms and value systems . . . ethos and habitus . . . [and] the 'subjective factor'" (p. 5) of professional life in modern Germany. As such, Franz's book reflects the most recent trend among historians toward more inclusive studies of cultures instead—or alongside—of structures. Here Franz is, by and large, correct that previous studies in the history of professions have not been so much concerned with experience as with structure, although most monographic case studies have inevitably, if not intentionally or primarily, revealed a great deal about the experiences of the human beings representing and being affected by the construction and exercise of professional power.

Moreover, Franz acknowledges she has not written a social history due to a scarcity of personal accounts. Her book is founded almost exclusively on published materials from various professional and general publications, along with some recourse to documents at the Geheimes Staatsarchiv in Berlin-Dahlem concerning state regulation of business education. Another limitation on the study, Franz claims, is the inaccessibility of documents from the archives of private firms, which means there is no coverage of the role of business administrators in industrial strategy making and rationalization.

Like many of the earlier works Franz criticizes, therefore, the core of her study concerns the "professionalization politics" (p. 11) of the business administrators' professional society, the Verband Deutscher Diplom-Kaufleute. Franz follows Tenfelde's argument that the German middle classes continued to strengthen and broaden their place in a modernizing society, but she also gives due consideration to the thesis presented, among others, by Claudia Huerkamp that the *Bildungsbürgertum* fragmented into "interrelated groups of experts" (p. 8) in line with the growing corporatism of capitalist society. She demonstrates that a majority of those with business degrees after 1930 were restricted to the accounting and bookkeeping departments of firms, failing to win a monopoly over business administration and finding themselves subordinated to engineers and other experts in the increasingly important—and American—fields of merchandising, advertising, and personnel management. The result, Franz says, is a case of "successful academization, failed professionalization" (p. 253).

This, curiously, seems like a very structural-functional conclusion to a work that professes to emphasize the cultural and subjective and may in fact not give sufficient credit to the breadth and depth of Franz's study of the *experience* of professionalization in this case. In fact, Franz goes on to conclude that the academic success of business administrators was in line with what she sees as their chief—and cultural (i.e., "subjective")—concern: the "'equality of rights' [*Gleichberechtigung*] of the economic middle classes with the 'educated classes' [*gebildete Stände*]" (p. 254). Thus, according to Franz, the protagonists in her study

were concerned as much, if not more, with social prestige as with professional monopoly and economic success. This changed only during the 1930s, when the economic middle classes had to compromise the social status *form* of "neo-humanistic educational ideals" (p. 257) through the *content* of marketable scientific expertise. For Franz, this is evidence of the continuity of a solidaristic hegemony of bourgeois ideals in twentieth-century Germany. Franz also concludes that it was the representatives of the discipline rather than the firms or the state who initiated the movement toward professionalization, but that, in Germany at least, state support for professional aspirations was essential. This mixed model is in line with the conclusions of the earlier research that Franz questions. And if, as Franz argues, the case of the professionalization of German business administrators is exceptional, then perhaps it is even less likely that this one case study could provide sufficient support for an ambitious thesis about the more general fate of bourgeois hegemony and solidarity in the twentieth century.

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MARTIN H. GEYER, *Verkehrte Welt: Revolution, Inflation und Modern München 1914–1924*. (Kritische Studien zur Geschichtswissenschaft, number 128.) Göttingen: Vandenhoeck und Ruprecht, 1998. Pp. 451.

In the century or so before the outbreak of World War I, Munich was famous primarily for its easy-going atmosphere, its annual beer festivals, its openness to outside influences, and its lively cultural and intellectual scene. Münchners liked to call their town "Athens on the Isar." Admittedly, this perspective was somewhat idealized, for the reality was rather darker and more complex. But even allowing for a myopia induced by too many evenings at the Hofbräuhaus, Old Munich doubtless offered a combination of quasi-liberal politics, lifestyle experimentation, avant-garde art, and congenial dissipation that was unique in Germany. By the mid-1920s, this happy image, however accurate it might have been, had changed dramatically; now the town was known for its arch-conservative political establishment, its extremely active *völkisch* organizations (including, of course, the Nazi movement, which was born there), and its cultural provincialism. Progressives in Berlin, the new and undisputed center of German modernism, lampooned the Bavarian capital as "the dumbest city in Germany."

Martin H. Geyer's new study helps us to understand this transformation in Munich's fortunes by closely analyzing the most crucial period in the city's modern history—the ten-year period between the outbreak of World War I and the restoration of monetary stability in 1924. Although the focus here is always on Munich, Geyer's object is also to use the Bavarian city as a means to illuminate changes on the broader German stage through which entire sets of values, conventions, and expectations were undermined without their being

replaced with a convincing alternative order. He calls this chaotic decade the "*verkehrte Welt*" (the world turned upside down). While not following developments in Munich or Germany all the way to Adolf Hitler's seizure of power, Geyer makes clear that the crucial steps down this road were taken in the immediate postwar period.

Much of the ground that Geyer covers will be familiar to students of twentieth-century German history. He revisits the "Spirit of 1914" that attended the commencement of hostilities; the escalating popular disillusionment and social polarization occasioned by wartime privations and mounting losses; the bitter antagonism between Munich and Berlin; Bavaria's amazing revolution, with its rapid passage from Kurt Eisner's befuddled Kantian idealism through the bizarre "soviet" led by Schwabing bohemians, to the bloody attempt to impose a true soviet by local Bolsheviks; the even bloodier "White Terror" that ended the revolution and made possible Bavaria's evolution into Germany's "order-cell" in the early 1920s; and, finally, the impassioned campaigns against the Versailles Treaty and the republican system headquartered in hated Berlin. What separates this study from the host of existing works on Munich's wartime and postwar experience is its meticulous and sophisticated analysis of the social-economic crisis that lay at the heart of the world turned upside down. More specifically, we have here a detailed look at how the German inflation in all its various phases and forms transformed almost every aspect of life in the Bavarian metropolis. The bitterness and pervasive insecurity engendered by the inflationary spiral—and not just by the "hyperinflation" of late 1923—undermined faith in traditional authority, promoting the growth of *völkisch* radicalism and the self-help politics of the various citizens' militias. The defeat of the political revolutionaries in 1919 by no means diminished the appeal of such groups, for they fed on a sense of outrage over the "war and inflation profiteers" who allegedly called the shots in Berlin. Even before Hitler tried to take over Munich as a springboard for his planned march on Berlin in the so-called Beer Hall Putsch of November 1923, Munich's rightist leaders had expelled eastern Jews from the city on the grounds that they were fomenting economic chaos for their own profit. Although he is familiar with the recently advanced argument that the inflation actually benefited the postwar German economy in certain respects, Geyer stresses that the contemporary perceptions of this episode were almost uniformly negative, and that the social-political implications were accordingly devastating.

For all his grappling with complicated financial and economic data, with changing labor markets and price indexes, Geyer never loses sight of the human dimension of his story—of the plight, for example, of small pensioners whose entire savings, and hence their sense of dignity, were swept away in the great disorder. This book contains, as it must, a certain amount of econom-

ic-historical theory, but the author wears his theory lightly and with mininial fuss. The result is a work that is at once analytically rigorous and thoroughly engaging, not an easy combination to achieve.

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ODED HEILBRONNER, *Catholicism, Political Culture, and the Countryside: A Social History of the Nazi Party in South Germany*. (Social History, Popular Culture, and Politics in Germany.) Ann Arbor: University of Michigan Press. 1998. Pp. viii, 317. \$54.50.

Based on impressive research in local and church archives, this interesting book by Oded Heilbronner examines the expansion of National Socialism in the predominantly Catholic Black Forest-Baar region of southern Baden, an area badly neglected by scholars. Like a number of other local and regional studies of the Nazi Party in both Catholic and Protestant regions of Germany, this work emphasizes the importance of the local "milieu" for the success or failure of the Nazi movement. But aside from Cornelia Rauh-Kühne's *Katholisches Milieu und Kleinstadtgesellschaft: Ettlingen, 1918–1939* (1991), this is the only major investigation of the relationship between the Nazi movement and the Catholic milieu in Baden before 1933. Heilbronner's work, which was originally published in Hebrew in 1993, examines Catholic and bourgeois milieus, but, in contrast to Rauh-Kühne, does not include Socialist organizations.

Heilbronner argues that as early as 1930, the Nazis won above-average votes in a number of Catholic communities in the region. The author gives the Nazi Party little credit for its success. The party achieved its first major electoral breakthrough in 1928 in northern, rural Protestant districts, but it did not establish itself in the Black Forest region until that year. He suggests that the Nazi electoral advance after 1928 was possible because of the erosion of the Catholic milieu, which began before 1914. By 1930, Heilbronner claims, barely half of the region's population belonged to that milieu. According to the author, long-term economic changes, combined with the collapse of "the entire Black Forest industrial economy" (p.25) between 1930 and 1932, further undermined Catholic and anticlerical bourgeois milieus.

Most important, according to Heilbronner, the breakdown of the region's bourgeois *Vereine* (voluntary organizations) permitted the Nazi Party "to penetrate the organizational vacuum without any special effort or technique and to win over large sections of the local Catholic society" (p. 3). Nazis succeeded first among anticlerical farmers, particularly after several peasant leaders joined the party in 1928. By 1930, workers and craftsmen, and, especially, former Socialist voters who had abstained from voting, were attracted by the party's socialist and anticapitalistic propaganda. New voters, coupled with the collapse of bourgeois parties, accounted for the massive increase

in Nazi votes in July 1932. Under the impact of the economic crisis, bourgeois sports and music *Vereine* lost members and were forced to reduce their activities. Their collapse, according to the author, induced the bourgeoisie to turn to the Nazi Party, because it "represented a way of escaping its socioeconomic and political isolation" (p. 191).

This book represents a valuable contribution to our knowledge of the Nazi movement in southern Baden and clearly demonstrates significant support for the Nazi Party in a number of Catholic communities in that region. In *The Nazi Movement in Baden* (1983), I noted that in 1930 the party "made significant inroads into the predominantly Catholic districts of Offenburg, Oberkirch, and Bühl" by reactivating voters who had not voted for the Catholic Center party (p. 190). But Heilbronner offers a much more comprehensive milieu explanation for the Nazis' success. Nonetheless, before 1933, the Nazi Party in Baden generally performed better in Protestant districts (*Ämter*) than it did in Catholic ones. The only district in the Black Forest in which the Nazis exceeded the state average in elections in both 1930 and July 1932 was Oberkirch, the most Catholic district (95.5 percent) in Baden. Curiously, the author has little to say about Oberkirch. In addition, Heilbronner reveals little about the activities of teachers in Baden's rural areas, even though in 1931 the Baden police reported that most rural teachers were secretly campaigning for the Nazi Party. From a comparative perspective, this is an important issue, particularly since Wolfram Pyta (*Dorfgemeinschaft und Parteipolitik, 1918–1933* [1996]) suggests that teachers in rural, Protestant villages helped Nazis penetrate the local milieu.

Heilbronner's conclusion that the Catholic milieu in Germany varied and "results differed from region to region, due to local conditions" (p. 154), is confirmed by Rauh-Kühne's useful survey, "Katholisches Sozialmilieu, Region und Nationalsozialismus," in Horst Möller, *et al.*, eds., *Nationalsozialismus in der Region* (1996) pp. 213–36, which appeared too late for his consideration. Heilbronner suggests that his findings are closest to those of Zdenek Zofka, *Die Ausbreitung des Nationalsozialismus auf dem Lande* (1979). But in contrast to Heilbronner, Zofka finds that Nazis in rural, Catholic Günzburg, Bavaria, were much more active in the *Vereine*, which was also, according to Rauh-Kühne, the pattern in Ettlingen in north-central Baden. Both Zofka and Rauh-Kühne also found little evidence of Nazi appeals to workers. In addition, church reports from Ettlingen in the spring of 1932 noted unusual religious zeal, which stabilized the Catholic milieu and was reflected in the relative weakness of the Nazi vote. But Peter Müller's dissertation on Catholic organizations in Konstanz (*Katholische Ständesvereine als Teil des Politischen Katholizismus* [1973]) documents that even a strong Catholic religious commitment did not always provide immunity against Nazi advances in some villages.

Heilbronner's suspect conclusion that the party suc-

ceeded "without any particular effort" (p. 234) is not shared by this reviewer or by Claus-Christian W. Szejnmann (*Nazism in Central Germany* [1999]). Szejnmann explains the success of the party in attracting workers in southwestern Protestant Saxony by focusing on the weakness of the Socialist milieu. However, he demonstrates that the party did best in several districts where it campaigned the most. Moreover, the party's general advance in southern Baden occurred only after the party began to organize and campaign in the region in 1928. And that applied not only to Catholic areas but also to the rural, Protestant district of Müllheim, where the Nazis' vote increased from 8 percent in 1928 to 30.7 percent in 1930. Nonetheless, Heilbronner has made a valuable contribution to the debate on the Nazi movement in Catholic communities in Germany.

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NANCY CARTWRIGHT, *et al.* *Otto Neurath: Philosophy between Science and Politics*. (Ideas in Context, number 38.) New York: Cambridge University Press. 1996. Pp. xii. 288. \$59.95.

We are like sailors on a ship forever at sea, obliged to repair our vessel and to improve its operation as best we can without the advantages of drydock. This figure of speech was popularized two generations ago by the Viennese philosopher Otto Neurath, who combined a robust embrace of uncertainty and historicity with an old Enlightenment belief in the liberating potential of science. This anti-foundationalist progressivism endears Neurath to those who find Jürgen Habermas's defense of "the uncompleted project of modernity" too Kantian, yet who also reject the deep skepticism toward science advanced under the signs of "poststructuralism" and "postmodernism." Nancy Cartwright, Lola Fleck, Thomas Uebel, and Jordi Cat offer a Neurath ideally suited to the needs of a turn-of-the-twenty-first century intelligentsia ready to advance again the old hope that intersubjective reason can be mobilized to improve knowledge and to inform democratic-egalitarian political initiatives.

This useful, attractive Neurath is the product of an ambitious collaboration among four scholars. Fleck is the chief author of the first one-third of the book, a sketch of Neurath's life from the time of his birth into a bourgeois Jewish family in 1882, through his involvement in the "Vienna Circle" of the early 1930s, down to his death in England in 1945, as a refugee from Adolf Hitler. Uebel has written the second one-third, an intensive examination of Neurath's half-dozen formulations of the "boat at sea." Finally, Cartwright and Cat reconstruct Neurath's ideas in terms that emphasize Neurath's differences with the "logical positivists" with whom he is often associated and maximize Neurath's ability to speak to issues as formulated by today's thinkers. The loving and detailed attention that the authors devote to Neurath's often imprecise arguments and to his winning but question-begging figures

of speech testifies to the depth of today's need for new ancestors.

Uebel's close interrogation of a series of specific formulations of the "boat at sea" is the best part of the book. Uebel analyzes Neurath's growing anti-foundationalism in relation to several specific contexts of argumentation. But Uebel does not address all of the relevant contexts. Neurath often sounds like the William James of *Pragmatism* (1907), which was widely discussed in Neurath's milieu just prior to the latter's first formulation of the "boat at sea" in 1913. Uebel deals with this potential connection to American pragmatism only in a speculative footnote. This narrow focus is all the more odd in view of the entire volume's explicit and sustained engagement with an idea routinely associated with the American pragmatists: that anti-foundational epistemological views and social democratic political views go together. It is no wonder that, in the late 1930s, Neurath found an ally in the aging John Dewey while getting a cold shoulder from the Frankfurt School emigrés.

The suspicion that the authors are engaged in special pleading is raised by their astonishing commentary on Neurath's perspective on politics. Neurath believed that social planning could go forward without reference to the politics that created the possibility of planning. He was an official in the revolutionary government of Munich in 1919 and was briefly imprisoned thereafter. His chief defense against charges of treason was that he was only a technician, doing economic planning for the radical regime but in no way concerned with its politics. This naïveté amused the worldly wise Otto Bauer and Max Weber, both of whom worked for Neurath's release. But Neurath continued well into the 1930s to claim that "the economic arrangements" for socialism "could be insulated from the volatile forces of politics and power" (p. 230). Ideology did not facilitate planning or indicate what direction it might take. Rather, ideology got in the way of pure planning. "Whether Neurath was right or not" in these matters, the authors declare with casual indifference to the issue, these beliefs "at least show" that Neurath "did not hold his claims about the interconnection of events as a necessary feature of empirical reality, that is, as a piece of a priori metaphysics" (p. 235). Such lapses at crucial points diminish the force of this book's hortatory conclusion, which calls on us in Neurath's name to integrate "will and cognition," to make the world whole through "action" (p. 256).

The impression persists that the real Neurath was even more interesting than is indicated by this well-meaning effort to make him "one of us." The authors might have served us better had they offered fewer of their own extrapolations and more of an account of what Neurath actually said. He was a truly remarkable man: a speaker with spectacular oratorical skills, an indefatigable champion of reasonableness and sanity in personal as well as public life, a political progressive, a builder of museums, an organizer of international

conferences, and a prophet of sensible theories of science that stress community, dialogue, and negotiation. He was a product of German-speaking Europe who grew to love England's very non-German way of "muddling through." Neurath left unfinished his greatest project, the *Encyclopedia of Unified Science*. He died suddenly amid a pleasant conversation with his wife, while reaching for, but not quite grasping, his copy of Goethe's *Iphigenie*.

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PAUL LAWRENCE ROSE. *Heisenberg and the Nazi Atomic Bomb Project: A Study in German Culture*. Berkeley and Los Angeles: University of California Press, 1998. Pp. xx, 352. \$35.00.

Werner Heisenberg, the inventor of quantum mechanics and a Nobel laureate in 1933, at the age of thirty-two, was one of the twentieth century's most creative physicists. "The end justifies the means," he would say, in justification of the unprincipled methods by which he tried to crack problems that others could not solve. His friend and sparring partner of the 1920s, Wolfgang Pauli, complained to their mentor and conscience, Niels Bohr, that Heisenberg, though brilliant, was "very unphilosophical." No harsher condemnation was available in the intellectual hothouse directed by Bohr, who had invented the quantum theory of the atom and directed its development until Heisenberg unphilosophically devised quantum mechanics.

In the 1930s, as professor of physics at the University of Leipzig, Heisenberg was attacked by the SS as a "white Jew" for teaching the non-German "abstract" physics developed by the full Jew Albert Einstein and the half-Jew Bohr. Heisenberg deflected the attack by appealing to Heinrich Himmler, to whom he had access through a family connection. The Nazis came to condone the teaching of relativity, without mention of its inventor's name, and Heisenberg enjoyed the patronage of the leader of the SS. He moved from Leipzig to Berlin as director of the Kaiser-Wilhelm Institut für Physik. There the main German "uranium project" had its seat.

The external development of the project is known in detail. By mid-1942, the technical problems in the way of making either a bomb or a reactor appeared to be too great, and the chances of success too small, to justify the necessary investment; or so Albert Speer, Hitler's minister of armaments and war production, determined when he decided not to proceed with a crash program. Heisenberg's team remained in the uranium business, however, working toward a self-sustaining chain reaction as a source of power in two or perhaps three senses. For one, as a literal prime mover; for another, as their postwar security as advisors in atomic energy; and, perhaps, as a weapon (a reactor bomb or runaway pile) if the technical difficulties of making one could be overcome.

The internal history of the project, the intentions of Heisenberg and his immediate associates, has been the subject of much contention. Heisenberg explained his involvement in a series of apologia that increased his virtue at every retelling. At first, he claimed only moral neutrality: he worked to discover whether a uranium weapon was feasible and was spared having to decide whether to make one by Speer's curtailment of the project. At the end, he claimed resistance: he had falsified his calculations in order to make the authorities believe that German science and industry could not produce the amount of explosive uranium (U-235) required. No doubt Heisenberg told his colleagues that a bomb required an amount of U-235 a hundred or more times greater than the quantity the allies found necessary, but it is more likely that he reached this conclusion by error than by treason.

Paul Lawrence Rose does not suffer any uncertainty in the matter. His Heisenberg would have developed a bomb if he could. Rose shows convincingly that Heisenberg made a serious but simple conceptual mistake about the limiting condition of an explosive chain reaction and that the strength of Heisenberg's confidence in his analysis kept him from undertaking the more detailed calculations that might have corrected it. A psychologist might see in this omission an unconscious effort to keep closed a question that Heisenberg did not want to open. Not Rose. He does not like Heisenberg (he says so himself) or Germans or what he takes to be a special German way of ignoring facts and escaping responsibility.

Rose's frankness may put off readers who think that historians should not approach their subjects prejudiced against them. In this case, however, both the admission of prejudice and the resultant portrait of Heisenberg are useful. In the last few years, Heisenberg's wartime behavior has been presented indulgently in two influential works: Thomas Powers's well-argued *Heisenberg's War: The Secret History of the German Bomb* (1993) and Michael Frayn's hit play, *Copenhagen*. Powers accepts Heisenberg's versions of the facts and so raises him from collaborator to saboteur. Frayn dramatizes the famous meeting between Bohr and Heisenberg in 1941 when Heisenberg went to occupied Denmark to lecture at a German "cultural" institution. Whatever was said at their meeting about the German nuclear project, Bohr always regarded Heisenberg's disclosures and overtures as self-serving and unfriendly. In order to make the episode good theater, Frayn introduced greater ambiguity into Bohr's behavior and more nobility into Heisenberg's character than the facts easily allow.

Rose's return to the unfavorable judgments of Heisenberg put forward by S. A. Goudsmit, a onetime colleague of Heisenberg's who rounded up the German uranium scientists during the last months of the war, thus has a timely value. It may, and should, have a long-term importance as well, as a counter to the

tendency to palliate the actions of collaborators in the Third Reich.

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PATRICK MAJOR. *The Death of the KPD: Communism and Anti-Communism in West Germany, 1945–1956*. (Oxford Historical Monographs.) New York: Clarendon Press Oxford University, 1997. Pp. xiv, 335. \$85.00.

For most casual observers, the history of German Communism ends with the Nazi burning of the Reichstag only to pick up again (if at all) with the establishment of the German Democratic Republic courtesy of the Soviet Union's occupation forces. Neither the resistance to Adolf Hitler nor the post-1945 attempt to rejuvenate the Communist Party of Germany (KPD) in the West have received the attention they merit—particularly in the English-language literature.

Utilizing a wealth of original sources, this astute and compelling study goes a long way toward correcting the neglect which the postwar western KPD has suffered. Patrick Major shows how the KPD was, for a time, both numerically and strategically stronger than the party of Weimar days. With membership at a historic high and solid influence at the center of German industry—among workers at Krupp steel, Ruhr mining, and the Hamburg docks, the KPD appeared poised to join the French and Italian Communist parties as a long-term player in postwar European politics. Yet, by 1956 the party was weakened to the point that the West German government outlawed it without inordinate protest.

What crippled the newly reborn party was the unlikely convergence of political repression by the Western powers with irrational dictates emanating from East Berlin. For different reasons, both sides of the Cold War had reason to want to see a weakened West German party. The United States and its capitalist allies had every reason to fear a radical party whose membership topped 300,000 by May 1947, while the Soviet Union and its steadfast East German Socialist Unity Party (SED) were concerned when they realized that West German Communist "cadres could not be switched on and off like a tap" (p. 40).

In order to insure loyalty to East Berlin, the SED purged the nominally independent KPD of over eighty percent of its leaders in 1951. A successful campaign was waged to replace the too self-reliant *altkommunisten* with younger cadres lacking the experience of working-class struggle during the Weimar Republic. One result was that local KPD leaders averaged a mere twenty-four years of age by 1954. Moreover, emulation of the SED saddled West German Communism with a top-heavy Stalinist bureaucracy that provided a functionary for every seventy-five members, whereas the Social Democrats had a mere 1:1500 ratio of functionaries to members.

But, as Major so cogently argues, the decline of the KPD can not be fully understood without reference to

antagonistic anticommunist forces in the West. Before World War II had even ended, the Western allies had developed contingency plans to deal with the likelihood of a powerful KPD. By mid-1948, leftists, not Nazis, were the prime target of U.S. intelligence efforts, which included forging a document claiming to be a KPD plot to sabotage Marshall Plan aid (p. 243). Nor was the party to suffer only from covert actions; completely legal KPD activities were impaired, at first, by the occupation authorities restricting access to paper and later by outright repression. For example, in the period 1946–1955, various Communist press organs were banned no less than 141 separate times. That many of the judges who ruled against the party were ex-Nazis caused widespread consternation, at least among civil libertarians, as the famous writer, Thomas Mann was driven to describe anticommunism as "the basic folly of our epoch" (p. 259).

Yet, Major shows that it was much more than "folly" that motivated the United States and its allies during the Cold War. Throughout this period, anticommunism was a reliable and widespread ideology that served any number of purposes. It allowed a close collaboration between former political enemies like the bourgeois Christian Democrats and the proletarian Social Democrats while providing an excuse to recreate a secret police system, now called Verfassungsschutz rather than Gestapo, and attack the popular peace movement—all in the name of defending democracy. The author points out that scholarly investigations into Communism during this period are commonplace, but anticommunism has evaded the same level of serious criticism.

This book reveals the complex interplay of factors that led to the decline of West German Communism while avoiding the Cold War stereotypes that have plagued previous studies. By fairly evaluating the influence of both sides of the Cold War, Major shows a sophistication all too uncommon when handling such a politically sensitive topic. This vitally important work provides a rich, masterly appraisal that will be unsurpassed for many years to come.

WILLIAM A. PELZ
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MASSIMO FIRPO. *Dal Sacco di Roma all'Inquisizione: Studi su Juan de Valdés e la Riforma italiana*. (Forme e percorsi della storia, number 3.) Turin: Edizioni dell'Orso, 1998. Pp. 232. L. 35,000.

Writers on early modern Italian religious history have operated in four main modes: Catholic, Protestant, lay, and, most recently, within the twin paradigms of social discipline and confessionalization. In this collection of six essays, as in his many other publications, Massimo Firpo approaches the subject from a decidedly lay perspective. He reiterates several major assertions familiar to specialists who know his work and may have seen these pieces in previous incarnations. His conten-

tions will stimulate those able to read Italian who have not already encountered them.

First, Firpo argues that the Reformation could take root only when and where "vigorous interventions and alignments of the political power" facilitated it (p. 61). This situation obviously did not obtain in politically fragmented Italy. After the Sack of Rome in 1527, rulers of Italian states could hardly ignore the religious priorities of powerful transalpine polities, above all the Holy Roman Empire. Nor could they afford to opt out of the papal patronage system, which supplied lucrative benefices for them and employment for their male progeny—not to mention, which Firpo neglects to do, honorable and relatively inexpensive placement in convents for their "surplus" daughters.

Second, Firpo maintains that the Reformation in Italy cannot be dismissed as a passive, mechanical reception of Lutheran, Zwinglian, and Calvinist ideas ill suited to germination in foreign soil. On the contrary, Protestant ideas from the north found fertile terrain in "autonomous [Italian] cultural legacies and institutional and social contests" (p. 80, 90), where they mutated accordingly. The adjective "autonomous" should not lead the inattentive reader astray: by using it, Firpo by no means intends to resurrect the claim by Catholic historians like Hubert Jedin, long superseded, that "Catholic Reform" occurred prior to and substantially independent of the northern Reformation.

Third, Firpo insists on situating the Italian Reformation in a Mediterranean context. In this regard, his key player is Juan de Valdés, a Spaniard of *converso* origin who escaped the Spanish Inquisition by fleeing to Italy, where in 1535 he settled in Naples. The attractive combination of *alumbrado* and Protestant motifs in Valdés's teaching and writing made a decisive impact, which persisted long after his death in 1541, on a host of prominent Italians, as well as on a highly placed English prelate resident in Italy, Reginald Pole. By energetically disseminating Valdés's ideas orally and in letters, circulating his works in manuscript, and eventually sponsoring their publication, these women and men, the *spirituali*, managed to "convert" many of their peers. Their last and greatest triumph was the *Beneficio di Christo* (1543), composed by the Benedictine monk Benedetto da Mantova and revised by the humanist Marcantonio Flaminio, both disciples of Valdés. Initially this little treatise, issued several times in the 1540s, struck prominent readers as a valuable expression of orthodox Catholic doctrine. For at least forty years after it was exposed in 1544 by Ambrogio Catarino Politi as a subversive amalgam of individualistic spirituality and Protestant concepts and then placed on the Index of Prohibited Books, the *Beneficio*, circulating in clandestine fashion, continued to introduce Italians to the charismatic Spaniard's ideas (p. 61–160).

On almost every page of this volume, Firpo engages in vigorous polemic with other interpreters of the Italian Reformation. Two instances of his wielding the

pen as a cutting instrument may interest readers of the *AHR*. In a review, appended to his second essay, of a volume edited by Jose C. Nieto, Firpo demolishes that scholar's defensive, contradictory attempt to reassert the exclusively *alumbrado* origin of Valdés's ideas (p. 82–88). Firpo's stiletto also stabs the proponents of social discipline, who he claims are trying, presumably for confessional purposes, to perpetuate the concept of "Catholic Reform" and the "myth of the Council of Trent" (p. 144–45). As noted earlier, the former is largely discredited; that the latter deserves further examination is beyond question. However, the insinuation that the paradigm of social discipline is an insidious Catholic historiographical ploy seems implausible.

In reviewing books by Italian scholars, I often conclude by lamenting that he or she will remain unknown to the English-reading public. Only one brief article by Firpo has appeared in English. An Anglophone press should commission a translation of his masterful, succinct, and accessible *Riforma protestante ed eresie nell'Italia del Cinquecento* (1993).

ANNE JACOBSON SCHUTTE
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CARLA FORTI. *Il caso Pardo Roques: Un eccidio del 1944 tra memoria e oblio*. (Gli struzzi, number 495.) Turin: Einaudi. 1998. Pp. vi, 287. L. 25,000.

At about 10:00 A.M. on August 1, 1944, a group of German soldiers burst into the home of sixty-eight-year-old Giuseppe Pardo Roques, president of the Jewish community of Pisa. The well-respected Pardo, a wealthy philanthropist who supported local Jewish and non-Jewish institutions alike, had declined to go into hiding even during the terrible last days of the German occupation of his city. An incapacitating phobia against animals made it difficult for him to live away from home. Also, he probably was relying on friendly relationships with local fascists who had protected him since the arrival of the Germans in early September 1943, in the wake of the Italian armistice with the Allies. On August 1, however, Pardo's luck ran out. After looting the house, the Germans murdered him and eleven others present in the house more or less by chance. The victims included Pardo's friends, neighbors, and domestic servants, some Jewish and others not.

Historian Carla Forti was not daunted by the fact that Silvano Arieti, an Italian-American psychologist, published a lengthy study of the atrocity, entitled *The Parnàs*, in 1979. Nor should she have been, for her work is much broader and deeper in scope. Drawing on archival materials and extensive oral testimony, she begins by dissecting the incident itself, asking minutely detailed questions not raised by the postwar police investigation. Many of her discoveries are original. She concludes, for example, that the Germans who killed Pardo were probably initially drawn to his house by

rumors of his wealth rather than by reports of his Jewishness.

Using the incident as a focal point, Forti proceeds to examine Pisan society before, during, and after the war. She analyzes the prewar position of Jews, finding it to be, for the most part, one of assimilation and acceptance. Like Pardo, most prominent Jews in Pisa were not hostile to fascism and urged coreligionists to maintain a low profile and refrain from dissent. Forti also studies the behavior of police and collaborators, describing them as harsh toward antifascists and some Jews but occasionally helpful to others who were important or personal acquaintances. She looks at accounts of the arrests of Jews in Pisa and its environs, and at descriptions of other massacres during the German occupation. This material is not new, but much that follows is.

Although the Germans responsible for Pardo's murder were never found, a local man was tried after the war for allegedly guiding them to the house. Forti examines documents from the police inquiry, the judge's pretrial investigation, and the trial itself. The result is a close-up of the Italian legal system in the immediate aftermath of the fascist regime. She analyzes the Bonomi government's legislation concerning sanctions against fascism issued on July 27, 1944, and asks why it did not define more severe punishments for individuals who denounced Jews. She finds the legislation unenforceable and ineffective, resulting in lighter sentences than would have been levied under the regular prewar penal law.

Perhaps the most original aspect of the book is the discussion of oral sources and the role of memory. About her witnesses, Forti asks the questions lawyers should ask. Was their testimony influenced by what they read or heard from others rather than by what they actually saw? Was it selective, distorted by personal preferences and prejudices? And about the Holocaust in Italy generally, Forti asks how it was remembered years later, by Jews and non-Jews in Pisa. Not unexpectedly, she finds that non-Jews usually recall that few Pisans were anti-Semitic, that most helped Jews in need, and that the deportations were entirely the fault of the Germans. More surprisingly, however, she discovers that Jews in Pisa also have selective memories. In the years immediately following the war, they had little interest in recording their Holocaust experiences. Even years later, they were reluctant to dredge up unpleasant memories. Most of those Forti spoke to did not mention local anti-Semitic incidents or their personal sufferings as a result of the anti-Jewish laws. These findings explain, in part, the generally favorable image of Italians during the Holocaust. They do not explain why Jews in other countries were less reticent about the offenses of their non-Jewish compatriots.

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JERZY LUKOWSKI. *The Partitions of Poland: 1772, 1793, 1795*. New York: Longman, 1999. Pp. xv, 232. Cloth £42.00, paper £13.99.

Jerzy Lukowski, author of *Liberty's Folly: The Polish-Lithuanian Commonwealth in the Eighteenth Century, 1697-1795* (1991), skillfully presents a "fresh, if necessarily, incomplete survey" of the diplomacy surrounding the Polish partitions (p. ix). He has not attempted, as he points out, the definitive study that would have required exhaustive study in numerous European archives. Such a study will be in order when the recently opened Russian archives present significant new evidence, but their study is just beginning and has yielded little as yet. Lukowski's account is heavily based on familiar published primary and secondary sources (mostly Polish). There is no material from Russian historical literature. Polish works published in the 1990s are also used; indeed, the book is dedicated to Łukasz Kądzioła, a Polish historian who died young in 1997. This work is not entirely synthetic, however. Lukowski cites documentation from Polish and British archives.

The book provides the first English-language account of all three partitions. Lukowski carefully puts partition diplomacy in the broader context of other diplomatic issues and Polish-Lithuanian domestic developments. He avoids historiographical commentary. The existing literature is mined for useful material but not discussed. Rival explanations are ignored.

Lukowski's study furnishes no major surprises. He confirms that Austria unintentionally touched off the First Partition by aggressive expansion in the Carpathians. Frederick II was all too happy to propose partition to Russia, and Catherine II agreed. Similarly, Prussia proposed partition during the Russo-Polish War of 1792, and Catherine agreed once more. The Third Partition became a foregone conclusion after the powers suppressed the 1794 Kościuszko insurrection. International considerations such as rivalries in the Balkans, rivalries in Germany, and relations with France played an important role in the diplomacy of all three partitions.

Through his narrative, Lukowski shows that Prussia was highly motivated to partition Poland but, for Russia and Austria, partition was merely a happy way of resolving diplomatic tensions. Prussia aimed to gain more territory and solidify its position in the East. Russia wanted some relatively minor boundary revisions at Poland-Lithuania's expense but preferred to control the rest of Poland provided that the Poles did not cause too much trouble. When they inevitably did, Catherine willingly partitioned the Commonwealth. Austria's desire to expand in the Balkans and combat the French Revolution outweighed its interest in maintaining Polish territorial integrity. As a result, Austria agreed to partition with little hesitation. Future historians will want to explore the motivations of the partitioning powers in greater detail based on a study of their domestic archives. Some other questions re-

main to be resolved. Most curiously, why did Catherine II hand Warsaw and all of central Poland to Prussia in 1794 after General Aleksandr Suvorov conquered it?

Lukowski criticizes all the Polish actors harshly. His chief villain is the Polish nobility, because its continuing attachment to the *liberum veto* and other Golden Freedoms prevented Poland-Lithuania from building a strong state structure. He is obviously correct that Poland-Lithuania was far too slow to awaken to the need for fundamental reform, although he goes too far in asserting that the cultural achievements of the late eighteenth century had little political significance. Lukowski is likewise too harsh on King Stanisław Augustus Poniatowski. He correctly observes that Poniatowski's major political concept, after his failure to reform the state in 1764–1766, was to obey Russia slavishly, yet the king deserves more credit for his cultural patronage and his organization of an influential Royal Party that dutifully enacted reform legislation in 1791–1792. Since Lukowski finds the opposition's policy of using Prussia to free Poland-Lithuania from Russia to be fundamentally flawed, he should be more sympathetic to Poniatowski and other reformers who saw no alternative to following a pro-Russian line. In general, Lukowski aligns himself with the so-called "Pessimist" school of Polish historiography against the "Optimists," who take a more positive view of late eighteenth-century Poland.

The book is well edited and well prepared. An appendix of manuscript sources for dietines between 1773 and 1788 will prove useful to a few specialists, even though it is not needed for reading this volume. Sources for the rest of the period, from 1764 to 1795, would have been useful, too.

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MARTA A. BALIŃSKA. *For the Good of Humanity: Ludwik Rajchman, Medical Statesman*. Translated by REBECCA HOWELL. Budapest: Central European University Press. 1998. Pp. xvii, 293. \$36.95.

Brilliant, opinionated, and relentlessly energetic, the Polish doctor Ludwik Rajchman was arguably the leading builder and activist in the field of international health during the first half of the twentieth century. Surprisingly, this is the first book-length biography of the controversial and visionary head of the League of Nations Health Organization (LNHO), who made enough enemies in high places that he was subsequently frozen out of the World Health Organization, and who retaliated by making health an ongoing concern of the United Nations International Children's Emergency Fund (UNICEF). It is a somewhat shortened English edition (financed by UNICEF) of Marta A. Balińska's *Une vie pour l'humanité* (1995). Balińska is Rajchman's great-granddaughter (although she never met him), and the book is a conscious tribute both to her own family and to the universalist aspirations of Poland's Jewish socialist intellectuals. She

interviewed or corresponded with more than a hundred former associates of Rajchman who had encountered him in a variety of contexts; their memories and opinions, as well as his private papers, constitute the hitherto untapped source material upon which the book is based.

Fourteen chronologically organized chapters follow Rajchman's career from his early involvement with the Polish revolutionary movement and his post-1918 leadership in the struggle against epidemics (first in Warsaw and then in Geneva) through the interwar years, when he divided his interests between Poland and Nationalist China, to his postwar leadership of UNICEF. Rajchman numbered among his friends such disparate figures as Thorwald Madsen, Karl Radek, Tzu-wen Soong (brother-in-law of Chiang Kai-shek), and Jean Monnet; moreover, his interests included microbiology, public health, humanitarian and international organizations, the fate of Poland, the emergence of modern China, and the formation of an integrated European community. He is thus a challenging subject for any biographer. Sadly, Balińska's reach almost always exceeds her grasp, and the result is a volume that provides far more information than insight.

The reader seeking to learn what made Rajchman tick soon grows uncomfortable with Balińska's questionable and unconvincing explanations. Although she claims that "his vision of the world would remain strongly marked by his Polish experience" (p. 27), the peculiarly Polish elements are never clearly defined, and when Rajchman encounters in Geneva a group of like-minded men, none of them Poles, who somehow share the same vision, the claim becomes downright flimsy. Nor are the sources of his ideas about international public health explored in sufficient depth. Experience in the bacteriological laboratories of the Pasteur Institute and the Royal Institute for Public Health may have taught him how to fight epidemics, but it scarcely explains his lifelong dedication to both social medicine and social reform. The author makes no effort to tap the intellectual or sociopolitical roots of the agenda that he proposed for the LNHO; did it owe as much to Edward Abramowski's "moral revolution," one wonders, as it did to Wycliffe Rose of the Rockefeller Foundation or to the innovative social hygienists who ran the Bolshevik Commissariat of Health Protection? Confronted by the rise of fascism, many left-wing Europeans—including Rajchman's brother Alexander—advocated a reconciliation between Communists and Socialists, but the idiosyncratic Ludwik—Lulu to his friends—attached himself to the cause of the Kuomintang (KMT), apparently because he regarded the Chinese as the first victims of fascism. The author never questions his equation of fascism with Japanese militarism, nor does she convincingly explain why a humanitarian internationalist should have put the KMT cause ahead of Ethiopia or Republican Spain.

A certain scholarly amateurism pervades the book.

So little use is made of relevant secondary literature that Rajchman's career is not properly placed in the appropriate historiographies. Nowhere is the reader given a sense that historians disagree over anything; one wonders whether the author is aware of major controversies, such as that sparked by Thomas McKeown over the relative importance of nutritional improvements versus public health intervention. Balińska's use of privately obtained verbal and written testimony also presents problems: often whole chunks follow one another, undigested and unanalyzed, sometimes without properly differentiated citations. Negative assessments of Rajchman—that he was “an almost blind Sinophile” (p. 99) or “a dictator by nature” (p. 232)—are either completely unexplored or insufficiently analyzed. Many of the family anecdotes, some almost embarrassing in their irrelevance, should have been omitted. Finally, the author leaves the impression that all of Rajchman's enemies were dinosaurs, imperialists, or anti-Semites, implying that there were no sound reasons for disagreeing with him. A satisfactory biography of Rajchman is, alas, still a desideratum.

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MARTIN MALIA. *Russia under Western Eyes: From the Bronze Horseman to the Lenin Mausoleum*. Cambridge: Belknap Press of Harvard University Press, 1999. Pp. xii, 514. \$35.00.

As a work of history, Martin Malia's book is rather an embarrassment. His kaleidoscopic survey of Western and Russian cultural history relies almost exclusively on evidence to support a single (albeit time-honored) argument that progress in the modern world requires the triumph of classical liberalism over the seductive and powerful forces of antirationalism past and present. It may well be that the great strength and contribution of this eloquently written book lie in the insights it provides into an intellectual mindset that prevailed in the 1950s.

Like many journalists, government advisers, and historians of the Cold War era, Malia views modern history as a Manichean morality play, with two “souls”—one rational, the other antirational and nihilistic—locked in mortal combat “over the body European” (p. 163). In Malia's dichotomized scenario, the liberal faith in “unilinear progress toward constitutional democracy and a rational economic order” is set in stark contrast to sinister, antirationalist forces that range from Friedrich Nietzsche and Karl Marx to the French Impressionists, Fyodor Dostoyevsky and “nihilists” like Lev Tolstoy.

As he sees it, liberals in the 1900s put Russia on track to become a constitutional, liberal order governed by a “civil society.” Historians will be surprised to learn from Malia that liberals led the Revolution of 1905, and—given the eruption of racist nationalism in today's Russia—they may wonder why Malia does not refer to the antecedents of the latter phenomenon in

the pogroms and right-wing terrorist organizations subsidized by the tsarist regime in the pre-World War I period. Instead, Malia focuses on Dostoyevsky's popularity among right-wing forces in nineteenth-century Germany and eloquently argues that both Russian and Western forces of cultural darkness were “the doppelgangers to our luminous modernity” (p. 165).

While he recognizes that important regional differences exist within Western European culture, Malia's conception of “difference” is quite hierarchical, with English classical liberalism situated at the top of civilization's ladder, Germany several rungs below, and Russia considerably lower. But he does not locate Russia beyond the pale (at least before the Soviet “aberration” of 1917–1991). Decrying Western tendencies to demonize nations simply for behaving like great powers, Malia argues that Imperial Russia engaged in “normal” imperial behavior, as the “Eastern front of the European conquest of the planet” (p. 413).

In Malia's account of the Soviet period, we leave the realm of history altogether to encounter a homogeneous and statically evil monolith. Scholars of the early 1920s will be astonished to learn from this book that by 1920 no autonomous social groups any longer existed in the Soviet Union. Malia ignores as well the deeply ambivalent Western response to the Nazis in the 1930s, the genocidal Nazi intention to eliminate “sub-human” Slavic peoples, and the 27 million Soviet war dead in World War II. He advances instead the peculiar argument that Adolph Hitler aided Joseph Stalin by making the Soviets appear democratic to a staunchly antifascist West and by making the “USSR the first power of Europe by starting and losing World War II” (p. 338).

According to Malia, the Soviet era constitutes the greatest mass hallucination in modern history,” engineered and manipulated by “the Great Khan”—a Stalin who, like the “Eastern” hordes that comprised the Red Army—was “incomprehensibly Oriental” (p. 370). While it may be that the use of such terms to describe and explain every conceivable sort of evil is attributable to Malia's sources, this reader was disappointed to find these cultural stereotypes so pervasive and so deeply embedded in his own, far more elegant prose.

The Soviet Union's demise and Russia's return to “normalcy” arouse in Malia some extraordinary—and to the diligent reader—rather surprising hopes for the future. After 400 pages devoid of economic history or analysis, we suddenly discover that “the predominance of private property and the market (that is, a ‘normal’ modern society) will in the long run produce the same effects in Russia that they have everywhere in the contemporary world: the formation of a civil society and a pluralistic culture” (p. 419). Like a wild card in a poker game, the universal laws of economic development—or what eighteenth-century liberals called

"the magic of property"—constitute for Malia the true guarantee of Russia's brighter future.

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P. A. KROTOV. *Gangutskaia Bataliia 1714 goda*. [The Battle of Hangö, 1714]. St. Petersburg: Liki Rossii. 1996. Pp. 247. \$15.00.

The reader of a book such as this must first bear in mind its context: part of the Soviet fleet was sold off by "entrepreneurs," and that which remains is rusting at Russian docks. The Russian navy initiated by Peter the Great three centuries ago has come to an inglorious end. This book by P. A. Krotov is a silent scream for remembrance of Russian naval tradition and its glorious beginnings.

Krotov has not written a patriotic rant, however, but a serious contribution to scholarship. His concern is one of the major events of the Northern War (1700–1721), which put an end to many of Sweden's pretensions in Europe while making it clear that Russia was an entity to be contended with. By 1714, Sweden was nearly bankrupt, and Denmark, which Russia was counting on for big-ship naval assistance in the Baltic, was bankrupt and consequently let Russia down by failing to send a promised eighteen ships of the line. Krotov says nothing about the state of the Russian economy in 1714 and gives the impression that Russia was doing rather well: Peter ordered eighty galleys, and they were delivered, armed, manned, and put to sea. Denmark asked for money and was sent some. Other scholars have pointed out that Russia also was nearly bankrupt by this time, but Krotov makes no mention of it.

In some respects, the battle of Hangö was trivial; in others, it was important as Russia's first major sea victory and perhaps a turning point in the Northern War. The battle was trivial in that a major Swedish frigate (pram), *The Elephant*, six accompanying galley ships, and two boats (all staffed primarily by untrained boys under the age of twenty) were trapped between the Gafvelsholm and Svedjeholm islands and the Padva Peninsula in the Rilaks-fjärd of Finland on July 27, 1714, by a Russian fleet of about ninety-eight galleys commanded by Peter the Great himself. The Russian ships encircled, boarded, captured, and took the prizes back to St. Petersburg for a victory celebration. The loss was less than ten percent of the Swedish navy, but it instilled such fear into the Swedes that they withdrew to defend Stockholm. The analogy is made that Hangö was the naval equivalent of the land battle at Lesnaia on September 28, 1708, in which the Russians defeated the Swedes prior to the 1709 destruction of Charles XII's army at Poltava. Hangö was not Poltava, however, because Russia lacked the big ships required to deliver the *coup de grace* to the Swedish navy or to mount an invasion of Sweden itself.

Krotov goes into every detail of Hangö. His source

base is impressive: ships' logs, memoirs, official Russian and foreign accounts, diplomatic and personal correspondence, court cases, regimental histories, award citations, service career narratives, and dozens of engravings. All that remains is submarine archeology of the battle refuse on the Rilaks-fjärd floor. Peter's galleys were of Turkish design with a very shallow draught but suitable for combat in coastal waters. A number of the Greeks, Slavs, and others serving on them previously had extensive Mediterranean service. Peter's role in Hangö is detailed, as is his merit as a naval strategist. Krotov describes the preliminary artillery engagement (which had no influence on the outcome) and then the boarding and hand-to-hand combat that resolved the battle. Casualties are discussed; especially interesting is the discussion of the death rate (both Russian and Swedish) from wounds. Numerous historiographic disputes are resolved in a convincing way. Pitiful letters in an appendix describe the plight of the Swedish prisoners of war, who were allowed to correspond only in German. Before July 27, the Russians built a corduroy road across the Hangö peninsula at present-day Sandö and constructed "sleighs" into which were loaded two galleys each trip for transport from east (the Tvärminne side) to west (closer to Rilaks-fjärd), but Krotov neglects to say how many galleys were so transported and what contribution they made to the battle.

After considerable discussion of the valor of the Russian forces at Hangö, Krotov ends his volume with a call for making the Sunday closest to August 7 (the modern date of the battle) a national holiday dedicated to the Russian navy.

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THOMAS M. BARRETT. *At the Edge of Empire: The Terek Cossacks and the North Caucasus Frontier, 1700–1860*. Boulder, Colo.: Westview. 1999. Pp. xv, 243. \$55.00.

This new history of the Terek Cossacks is doubly welcome. The subject is relatively neglected, and the present situation in Chechnya gives it current relevance. For almost half a millennium now, the north-eastern slopes of the Caucasus have been the scene of intermittent conflict and exchange between Russia and Asia, Muslim and Christian, an inspiration to poets, an arena where the "Great Game," the ongoing struggle between the West and Russia, is played out.

Cossacks settled along the Terek River after Ivan IV conquered Astrakhan, but evidence of their early history is scanty. The earliest documentary reference is dated 1563, but formal enrollment began only in 1720, and it is then that this history begins in earnest. Basing his account on material in the Russian State Historical and Military History Archives, local newspapers and journals, as well as secondary sources, Thomas M. Barrett's approach is primarily topical. After brief accounts of the Terek Cossacks' origins and colonization of the frontier, he deals in detail with their

interactions with the local environment, their economy, cross-frontier commerce, gender relations, military service, ethos, and culture. Demography, including inward and outward migration, and Cossack identity and loyalties also receive attention. The approach reflects positive trends in contemporary historiography, including a willingness to learn from other disciplines, and some of its shortcomings too.

Given the spottiness of the available data, Barrett's account of environmental, and ecological factors is particularly commendable. Strong on soils, cultivation, forests, and flood control, the book also provides useful accounts of malaria and of a locust plague. Cossacks' cultural adaptation to environmental conditions is demonstrated in respect of housing, clothing, and means of transportation; secular geographic changes are recognized (in the behavior of rivers for example), although the impacts of climatic change and forest fires are overlooked.

Barrett has also tried to reconstruct the local economy and provides some data on (though no thoroughgoing treatment of) the Cossack household economy. His treatment of cultural loan is more successful. Implicitly following Owen Lattimore's precept that frontiers are zones of exchange rather than lines of division, Barrett provides an interesting chapter on trade. Smuggling, the ransoming of prisoners, and trafficking in slaves and plunder, as well as more mundane commodities such as salt, fish, weapons, wood, and clothing, were all part of the Cossacks' commercial transactions with their neighbors to the south. But as Russian colonization of the hinterland increased, the direction of Cossack commerce shifted to the steppe zone to the north, and frontier exchange became a matter of culture rather than economics.

In a chapter cheerfully entitled "Seven Brides for Seventy Brothers," Barrett argues that the exigencies of Cossack life made for singularity in the status of Cossack women. They did most of the work, but, so far from conforming to the popular stereotype as victims of patriarchal oppression, they were commonly as old or older than their husbands, relatively free in sexual behavior, held the purse strings, and outlived the men. Nor did this freedom disappear when the imbalance between the sexes was repaired. Men were expected to be models of courage and, especially if they died in battle, would be commemorated with a series of funeral and commemoration feasts.

In his examination of warrior culture, Barrett invokes both the Senj Uskoks and Fernand Braudel's last work. Yet the comparisons implied are not always apposite, and there are other sources that might be: for example the several studies of medieval frontier societies. Furthermore, genetic studies and analysis of personal names may yet throw more light on the ethnic origins of Terek Cossacks.

The book contains some questionable interpretations. What Barrett takes to be the "exaggerated Muscovite manner" (p. 20) was, in fact, a recognized way of establishing a territorial claim; the ransoming of

kidnap victims was not only a formal means of conflict resolution but also a source of income. Furthermore, the Russians had to suppress the primitive law of blood feud if they were to bring order to the northern Caucasus. The alternative, as we have seen in Albania recently, is a state of anarchy that does not respect frontiers. Nor was General A. P. Ermolov crueler than others who subdued native peoples in the nineteenth century. In suggesting otherwise, Barrett recalls current Western prejudices.

These reservations excepted, Barrett is to be congratulated for bringing together so much information about the Cossacks of the Terek and their relations with the northern Caucasus and its peoples and for presenting it interestingly. The book is well written and well equipped with notes, maps, illustrations, and a brief chronology.

PHILIP LONGWORTH
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DAVID ALAN RICH, *The Tsar's Colonels: Professionalism, Strategy, and Subversion in Late Imperial Russia*. Cambridge: Harvard University Press, 1998. Pp. xiv, 293. \$49.95.

A handful of highly trained, talented general staff officers strongly influenced Russian military and foreign policy beginning in the 1860s. By means of their professional expertise, they effectively limited the power of the emperor. So argues David Alan Rich in this archivally based, thought-provoking study. Its protagonist is Nikolai Obruchev, a little-studied career military officer who, although in close relation during the early 1860s with antigovernment activists, three decades later almost single-handedly engineered Russia's strategic embrace of France.

The book's main focus is the rise of professionalism in the Russian general staff (part one) and its impact on strategic planning (part two). Rich contends that, because of their political background and authoritative and surprisingly autonomous activity at the highest levels of government, staff officers like Obruchev had a subversive impact on the autocratic political system. Professional culture, "a faith in the mastery and efficacious application of systematic knowledge" (p. 30), made its first appearance in Russia within the War Ministry, whose researchers began in the 1830s to lead "the government in the drive to 'know' the Russian empire" (p. 45). Systematizing masses of statistical and interpretative data and formulating sophisticated war plans, general staff officers developed an ethos of professionalism that arose only subsequently in other areas of governmental and public life. In this connection, Rich explicitly rejects the argument that professionalization in Russia grew out of civil society.

The new military planners, stimulated to concerted activity by the Prussian defeat of France in 1870, were put to the test in the war with Turkey in 1877-1878. Rich shows that the Russian general staff began to plan for the war in early autumn 1875, not a year later

as commonly assumed, which permitted Russia to deploy its military forces with surprising rapidity and to press hard for concessions from Turkey and the European powers. The only problem was that the specialized military planners failed to place war making in its broader political and diplomatic contexts. This failing, which resulted from inordinate reliance on specialized strategic expertise, undermined the whole of Russian foreign policy. It led Russia's rulers to exchange their traditional, and more sensible, alliance with the central powers for one with republican France. Most important, Obruchev's strategy to lure Austria into an expensive engagement in the Balkans ultimately helped bring about World War I. Obruchev was the "precise analog" of Alfred von Schlieffen, according to Rich, because both conceived short-sighted military strategies that made it hard to avoid protracted, multinational war. Finally, the Russian general staff's obsession with strategic planning prevented it from assiduously increasing the military's combat effectiveness.

The book's sustained analysis of strategic developments in Russia runs from the appointment of Dmitry Milyutin in 1861 to the strategic shift in the late 1880s, with only a cursory examination of developments thereafter. He states sweepingly that Russia's military planners fell into decline beginning in the 1890s, yet supports his contention with little evidence. Rich occasionally contradicts himself or, at the very least, writes ambiguously. General staff work, as a development in military technology, was "as important as big-gun battleships" (p. 30), and the general staff professionals were of "stunningly high caliber" (p. 42). Still, they "were not the saviors of Russia but the virtual guarantors of its demise" (p. 19). The result of all their preparation for war making is also in question. "There can be no doubt," Rich writes, "that by the late 1880s the empire could assemble, transport, and concentrate an army . . . at least on paper" (p. 209). At one point Rich argues that Germany had no hostile intentions against Russia, yet further on he confesses that he is "powerfully attracted" by Fritz Fischer's argument that Russia was facing "subordination, if not virtual vassalage, to Germany" (p. 279). The sense of progress that concludes nearly every chapter is invariably dashed by a pessimistic assessment of the Russian military's continued backwardness in the first paragraphs of the next chapter.

Rich's attribution of enormous policy-making influence to general staff officers demands a revision of the standard interpretations of the nature of Russian autocratic government in the late nineteenth century. His insights into the unfolding of professionalization in Russia provoke reflection on the nature of Russia's civil society. Historians of late imperial Russia and military historians generally, as well as political sci-

tists and sociologists, will benefit from reading this book.

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OLEG KHARKHORDIN. *The Collective and the Individual in Russia: A Study of Practices*. (Studies on the History of Society and Culture, number 32.) Berkeley and Los Angeles: University of California Press. 1999. Pp. xii, 406.

In this beautifully designed, provocative book, Oleg Kharkhordin seeks to explain how a presumably collectivist society, largely bereft of fully formed individuals, generated during the Soviet era the "adoption . . . of all 'core ideas' of Western individualism" that the author claims characterizes contemporary Russia (p. 358). "Small pragmatic changes made possible the whole-hearted support of such novel values as privacy and autonomy," the author tells us, "while self-development was a value in the Soviet discourse since its inception." "The Soviet attention to the individual . . ." he continues, "was one step away from the assertion of the unconditional self-worth or dignity of the individual as such" that allegedly reigns in Russia today (p. 358).

One must accept the author's "givens"—the absence of "individuals" among the Russian "masses" before the Soviet era and the adherence by Russians today to Western notions of selfhood (itself a reified notion)—for the rest of this book to be credible. In distinction to the West's path to "individualization," which Kharkhordin, following Michel Foucault, attributes to the religious practice of private confession, Russia's road to creating "individuals" was paved by the practices devised by 1920s educator Anton Makarenko, inspired (Kharkhordin strongly suggests) by sixteenth-century Russian monastic practices of admonition and public penance, with expulsion and excommunication as punishments of last resort. Kharkhordin writes that Makarenko was not trying to create the Orwellian mass man but simply orderly individuals out of chaotic masses. "In other words, his concern is not with producing conformity among individuals, but with producing individuals at all" (p. 201).

The ubiquitous Soviet collective's role in the emergence of self-fashioning is at the heart of the story. Thanks to the continual scrutiny of the collective (*kollektiv*, or primary affinity group), the "self was made an object to care about, to reflect upon, to perfect" (pp. 4–5). Yet, by Kharkhordin's own admission, most Soviet citizens recognized the sham nature of the criticism and self-criticism mandated by the rules of collectives. Self-fashioning in this venue consisted of perfecting methods of dissembling, evading responsibility, and egoistically privatizing community resources, certainly not developing the inner self and autonomous values of the "Western" post-Kantian self.

Perhaps the most disturbing aspects of the book are the attempt to reframe the Stalin era as one that enabled freedom to develop and the parallel denigration of the Khrushchev period as one of freedom's curtailment. Amid the terror, we are told, "Stalin's regime still allowed for the existence of random patches of individual human freedom . . . people could sometimes escape the imminent threat of terror by simply moving to another city, by making a brilliant rhetorical counterblow during the purge assembly meeting, by turning the tables on their persecutors" (p. 302). In contrast, Nikita Khrushchev's more efficient, less chaotic forms of social control erased that "space of uncompromised human freedom and dignity" that coexisted with Joseph Stalin's terror (p. 302). "Why," Kharkhordin asks, "wouldn't this epoch of *kul't lichnosti* . . . 'the cult of personality' (that is, of Stalin's persona), be better understood as the time of the cultivation of each single individual, in Makarenko's phrase?" (p. 230).

Why *wouldn't* it? Because it *was* a time of unprecedented mass repression of individuals, their autonomous visions, and their claims to an independent analysis of reality. And if Kharkhordin thinks that it *should* be understood otherwise, he should say so. Moreover, although Stalin's terror may indeed have been less pervasive than Khrushchev's routinized social control, that still does not mean that individuals experienced more freedom under Stalin than under Khrushchev.

Wasn't the collective and its mutual surveillance less about "instruction" and fashioning of new selves and more about selecting victims, as Aleksandr Zinoviev asserts? Wasn't it less about how self-developed one was and more about whether one was "in" with the dominant clique? Kharkhordin later agrees, now arguing that in the post-Stalin era a "private sphere" emerged as Soviet citizens attempted to shield their lives from the collectives to which they were attached (p. 270). And, we are told, it is through this retreat into privacy that *Homo sovieticus* finally emerges as an "individual" (p. 272). But again, how was this Soviet dissembler any different from the nineteenth-century Russian peasant or bureaucrat who dissembled to the landlord or to the tsar's governor-general? Are we dealing with anything novel, or simply with repetitions under new names?

Kharkhordin's genealogy seems far-fetched as well, not least because monastic admonition, public penance, and expulsion were decidedly marginal cultural and religious forms by the late nineteenth century. It is certainly much more likely that the model for Makarenko's collective came from peasant society, where the tsarist authorities for centuries imposed *krugovaia poruka*, or collective responsibility, for actions of individual members of the commune, and which consequently encouraged monitoring by all peasants of all their neighbors. This ties in with the peasant conviction that there were limited resources to go around, and therefore one needed to watch jeal-

ously that one's neighbor did not get more than his or her allotted fair share. Mutual surveillance, Kharkhordin admits, was "practiced for centuries of rural life, it was part and parcel of the Russian peasant land commune" (p. 110). Why, then, make the monastic rule so central to the story? That raises the question about Foucault's own genealogy of "Western" selfhood. An argument that asserts that a more variegated and expanding technical and commercial society required individuals who were capable of making informed decisions on the basis of their own, autonomous judgment and expertise seems more plausible than a decision of the Lateran Council of 1215. In fact, Anatolii Grigor'evich Vishnevskii, in *Serp i rubl': Konservativnaia modernizatsiia v SSSR* [The Sickle and the Ruble: Conservative Modernization in the USSR] (1998), makes just this argument to explain whatever stunted individualism did emerge from the Soviet era.

Kharkhordin's treatment of pre- and post-Soviet Russians as a single, undifferentiated mass surprised me; it is never completely clear whose experience, concretely, is being examined. The evidentiary base for the author's expansive thesis consists largely of a select number of existing sociological studies and works of fiction whose representative nature is not demonstrated. And although the book contains one of the most lucid discussions of Foucault's method that I know, the self-consciously Foucauldian approach wears thin, particularly because the genealogies are so unconvincing. I wish that the author had held broader discussions with historians before publishing his work, given its deeply historical nature. Nevertheless, this book, despite its flaws, is sure to stimulate debate about the socializing roles of Soviet-era institutions. Kharkhordin merits our recognition for helping to open up critically important study of the nature and construction of selfhood in Russia and also for placing the question of everyday practices on the front burner of Russian studies.

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R. W. DAVIES. *The Industrialisation of Soviet Russia*. Volume 4, *Crisis and Progress in the Soviet Economy, 1931-1933*. New York: Macmillan. 1996. Pp. xviii, 612. £70.00.

This volume by R. W. Davies covers just three years, which saw the Soviet First Five Year Plan give way to the Second as the leadership moderated heady enthusiasms ("the five-year plan in four years!") in favor of the painful acknowledgment that reality could not be defied forever. As in the previous volumes of this series, the depth and scope are remarkable, yet a light style, even occasional suggestions of humor, produce a text that seems exhaustive but not exhausting. A reader seeking conclusions in the conclusion will be disappointed, but the summary that masquerades as a conclusion is masterly, and for some nonspecialist readers, this will be enough. The cross-headings, in-

dexing, and constructive use of footnotes make life easy also for the dip-and-skim reader. As for conclusions, these are to be found scattered within the chapters, sometimes implicit rather than explicit but all plainly with a strong archival foundation.

As the First Five Year Plan came to its end, the sacrifices and grotesque imbalances that supported the investment in large-scale industry still had not resulted in new plants that were anywhere near to coming into full production. But new technologies were being mastered, although at excessive cost. This was a time of rationing, with even the treasured possession of a favored-category ration card being no guarantee against starvation (details of the concurrent agricultural crisis have been reserved for a forthcoming volume of this series).

So although Russian industry did take a great leap forward, there was an accompaniment of misery and waste. The leadership was willing to take advice, but that advice often conflicted. Joseph Stalin, with his robust commonsense, often made good choices, but his was not the only voice. Politburo members had their own axes to grind, and the opinion of the party as a whole had to be taken into account. Past slogans and ideologies could still stand in the way of sensible options. Trying to please everyone, or almost everyone, was too often the line taken. As Davies points out, the Soviet system *cannot really be defined by any of the conventional totalitarian models*.

This book leaves the impression that the protracted policy arguments, inside and outside the Politburo, resulted only in an excess of wrong turnings. Perhaps this was because so many of those long convoluted arguments raged around the choice of which banana skin to tread on next. Mismanagement was most evident in what the planned economy had been expected to eliminate: the mismatching of resources. Ration cards that were not backed by available food supplies was what the ordinary citizen cared about most, but there were many other failures to cope with the interrelationships of an advancing economy. The vast Dneproges hydroelectric plant was completed promptly but lacked customers for its power. Chicago-style meat processing plants were expensively completed just as the collectivization drive put an end to the cattle stocks on which they depended. An electric power station was brought to its knees by a shortage of horse fodder (!). And on top of problems like these, there was a world Depression that cut the value of grain and timber exports and a Japan that was openly discussing the best date for an invasion of Siberia.

Rearmament, internal and external trade, labor, finance, transportation, consumer industry, and foreign specialists are among other topics dealt with at length in this book. In each of these, there were occasions when the USSR made experiments that, even in failure, are worthy of study. Sometimes, of course, what the Russians discovered may already have been well known elsewhere—like the discovery that when workers' rewards move from time rates to vol-

ume rates, the unskilled respond markedly whereas the skilled produce little extra (a situation not without its parallels in academic circles). As for foreign experts, they were vital and prized, but toward the end of this period they were being dispensed with, usually prematurely, to save foreign exchange.

This book is a worthy fourth volume to the "Industrialisation of Soviet Russia" series. It is surely destined to become the standard work on its subject and it seems strange that this particular volume has to be obtained through its British publisher, no U.S. publisher having taken it on. Evidently the premature dismissal of overseas experts is not a uniquely Soviet phenomenon.

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YITZHAK M. BRUDNY. *Reinventing Russia: Russian Nationalism and the Soviet State, 1953–1991*. Cambridge: Harvard University Press. 1998. Pp. x, 352. \$45.00.

This is a timely book. In 1991, only five deputies in the Russian Soviet Federated Socialist Republic (RSFSR) Supreme Soviet voted against the Belovezh Forest accords that dissolved the Soviet Union. In March 1996, however, the Russian Duma voted by an overwhelming majority to annul the decision of its predecessor. Boris Yeltsin won the June–July 1996 presidential election, but it was significant that Gennady Zyuganov did not run as a communist but as the leader of a communist-nationalist alliance, the Bloc of National-Patriotic Forces of Russia. By late 1999, Vladimir Putin, the prime minister, had become the leader of the nationalist-patriotic bloc, pushing aside the communists. Liberals and liberal nationalists who had carried everything before them in late 1991 now found themselves marginalized. Yitzhak M. Brudny's scholarly study provides the background to this remarkable transformation.

A major reason for the rise of Russian nationalism was the shortcomings of Marxism-Leninism as the ruling ideology. From 1953, the state coopted it as a legitimizing mechanism but the distinction between Russia and the Soviet Union remained a problem. Gradually Russians dominated the ruling elites, and Russians came to regard the Soviet Union as coterminous with Russia. Unlike other federal republics, Russia did not have its own Communist Party, academy of sciences, etc. until the late Gorbachev era. Hence Russians did not regard themselves as having an empire.

Brudny divides Russian nationalists into three groups: liberal, conservative, and radical nationalists. Before 1985, these labels referred to the views of individuals and literary journals. During the Gorbachev years, they also applied to weekly and daily newspapers, cultural organizations, political movements, electoral alliances, and parliamentary factions in the Russian and Soviet Supreme Soviets.

Liberal nationalists split from Russian liberals because they rejected the liberals' preoccupation with individual human rights and the adoption of a Western-style market economy. Liberal nationalists advocated far-reaching political and economic reform and the wholesale rejection of Stalinism and its legacy. In sharp contrast to the conservative and radical nationalists, liberal nationalists did not idealize the Russian village as the fount of Russian moral values. They were not anti-Semitic or anti-Western. They enthusiastically supported Mikhail Gorbachev and then Yeltsin when he assumed the leadership of the liberal reform movement. The core of this support was Democratic Russia, which played a key role in Yeltsin's presidential victory in June 1991. Liberal nationalists could never make up their minds about whether to keep or destroy the Soviet Union. They had a positive attitude to the tsarist past and, of course, were totally opposed to the August 1991 attempted coup. Before *perestroika*, their views were mainly represented in the liberal wing of the village prose writers, in *Novy Mir*.

The conservative nationalists were closely allied to the liberal nationalists in the 1960s but thereafter moved toward the radical nationalists and those communists who rejected the reforms of the 1980s and 1990s. The conservative nationalists idealized the Russian peasantry and its moral and cultural values. They regarded the renaissance of the peasantry as essential to the regeneration of Russia. In the 1950s and 1960s, these writers concentrated on the hardships suffered by peasants during collectivization and after. In the 1970s, as living standards rose in the countryside, conservative nationalists pilloried the moral and economic corruption of urbanization and modernization. Predictably, they became fierce opponents of the Gorbachev reforms and allied themselves to the radical nationalists, orthodox communists, and those in the military and KGB who tried to stem the tide of democratization. They were strongly opposed to the penetration of Western values in Russian life. Their ideas were articulated mainly in *Moskva*, *Nash Sovremennik*, and *Literaturnaya Gazeta*.

Radical nationalists advocated a strong, authoritarian state to preserve Russian values. They vigorously struggled against the inflow of Western values. They greatly admired Joseph Stalin, and they measured leaders in the 1970s and 1980s against him and found them failing to protect and strengthen Russia. Economic and political liberalization was regarded as undermining the Russian state, and they loudly advocated the removal of Gorbachev and then Yeltsin. They failed to achieve this, but their ideas were gradually adopted by radical nationalist political movements. Their main journal was *Molodaya gvardiya* and under Gorbachev, *Pamyat'*, Vladimir Zhirinovskiy's Liberal Democratic Party, and newspapers such as *Den'*.

Brudny has ploughed through "thick" journals, newspapers, and novels to produce an impressive study of the multifaceted phenomena of Russian national-

ism. He had access to some KGB files, and this lends greater depth to his. Brudny's next task, one hopes, is to chart the incredible fall and rise of radical nationalism under Yeltsin.

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DOUGLAS R. WEINER. *A Little Corner of Freedom: Russian Nature Protection from Stalin to Gorbachev*. Berkeley and Los Angeles: University of California Press. 1999. Pp. xiv, 556. \$55.00.

Douglas R. Weiner explores the fascinating vitality of nature protection movements in the Soviet Union from Joseph Stalin's time to the end of the Gorbachev era. In a provocative study based richly on archival materials and interviews with a number of the actors in the movement, Weiner shows how even in an authoritarian regime environmentalists were able, in their professional societies and research institutes, to preserve a modicum of independence of thought and activity that ran counter to state programs. The symbolic and intellectual corner of freedom for scientific activity consisted of nature preserves called *zapovedniki*, which comprised less than 0.1 percent of Soviet territory yet provided the scientists with a sense of identity and mission.

In Stalin's Russia, party officials and economic planners saw *zapovedniki* primarily for their economic use. Weiner describes the constant struggle of the environmentalists to save the *zapovedniki* from economic encroachment. Weiner examines the extent to which nature protection served state goals, indeed questioning the degree to which the apparently autonomous nature protection movement served interests of the state. The answer is very little, given the proclivity of Stalinist planners to judge the value of nature solely in terms of its contribution to state economic goals. Agricultural and forestry interests in particular pushed harvesting and logging in the *zapovedniki*. If "acclimatization" of the reserves ended in mid-1960s, poaching, recreational abuse, and harvesting of resources continued. Against the backdrop of the environmentalists' effort to defend their little corner of nature, Weiner also shows us the major events in the history of Soviet environmental protection: industrial encroachment on Lake Baikal, Siberian river diversion, Kedrograd, and Lysenkoism. Regarding the last, many scientists purged as a result of the rise of T.D. Lysenko found refuge in *zapovedniki*, so he exacted revenge on them through attempts to destroy the preserves.

Weiner is careful to point out that the conservation movement did not contain the seeds of civil society from which a democratic body politic might grow. Rather, environmentalists opposed some aspects of the system but were part of others, and they used what spaces they could to work within limits of system, in this case the sacred space, intellectually and physically, of the *zapovednik*. They offered no clearly articulated critique of the system, nor a unified movement. Mem-

bers belonged to a variety of institutes and societies that serve as the focus of this book. But because of their persistent efforts to save the *zapovedniki*, naturalists were the only group of persons fearless and mobilized enough to face down anti-*zapovednik* sentiment in the years after Stalin's death.

Weiner's study raises a series of questions which must be answered about the ability of the Stalinist regime to control speech and suspect political other activities. Why did the regime tolerate a seemingly subversive movement? Did its leaders not see its political content (or expect it) in the struggle to protect *zapovedniki*? It remains surprising that political commissars saw environmentalists as eccentric but not harmful, and their activities as apolitical, while they required other groups of scientists—physicists, chemists, and most biologists—to bow to political and economic pressures.

By 1965, elite biologists, followers, and students realized that they represented a reasonably autonomous, cohesive movement opposed to regime economic development policies in environmentally fragile areas, but they also understood that the corrupt Soviet system would always protect those who violated laws in support of economic development. For this reason, they worked through the Moscow Society of Naturalists, which was connected with Moscow University, and not the All-Russian Society for the Protection of Nature, the largest nature organization in the world but one that had been coopted by economic interests.

Weiner is critical of the naturalists' efforts to preserve *zapovedniki* as inviolable biological units in the face of growing scientific evidence to the contrary. Yet, as he points out, those nature preserves were all that they could maintain as what he calls "a geography of hope" or an "archipelago of freedom" within the Stalinist system. They had no sense of the relation between the transformation of nature—itsself promulgated in a government resolution in 1948—and the way the regime reproduced itself. He also criticizes the castelike, elitist position of scientists in Stalinist society, in spite of their professional ethical impulse and duty to serve nature. All this may explain the rapid marginalization of the environmental movement in the Gorbachev era and beyond, when purely economic and political issues such as privatization and unemployment overwhelmed environmental ones after a brief period of local public activism against biochemical facilities, fossil fuel plants, and nuclear power stations.

Some readers will bog down in the encyclopedic detail of persons, institutes, and geographic locations. Conservation history, as the author tells us, is awash in conferences, and Weiner takes us to all of them. For that reason alone, I urge Weiner to consider a shorter book, perhaps a university textbook covering this book and his earlier one, *Models of Nature: Ecology, Conservation, and Cultural Revolution in Soviet Russia* (1998), so that many more persons will be able to share in this marvelous story and use it in courses on civil society in

authoritarian regimes, Soviet history, and environmental history.

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MIDDLE EAST AND NORTHERN AFRICA

NATHAN J. BROWN. *The Rule of Law in the Arab World: Courts in Egypt and the Gulf*. (Cambridge Middle East Studies, number 6.) New York: Cambridge University Press. 1997. Pp. xvii, 258. \$59.95.

Nathan J. Brown focuses his account of rule of law on the development of the modern national court system. In doing this, he takes as his examples the courts of Egypt and of the Gulf States. Brown's work thus fits into the wider field of studies of law in the modern Middle East, a field encompassing studies of both religious and secular institutions, lawyers, and case law—especially now in women's studies, a field that has yet to grapple with theories of penology and jurisprudence—or with the prison population. Brown's study is built on this wider terrain; it includes two final chapters of generalities, one on business and the court, the other on "popular uses of the court."

The focus of this book explains the choice of source materials, which add up to papers drawn from the Public Record Office in London and from the National Archives in Washington, D.C., along with newspaper articles and interviews with living personalities. Local case laws, legal journals, law school research, and Egyptian third-cycle dissertations in law from France are cited but play less of a role.

Brown begins his work with a chapter reviewing different views on state and law found in American social science. He prefers the one that law serves state interest over the view of law as a check on states. Brown (correctly, in my view) postulates that the development of rule by law is a part of and a window on the hegemony. With this as a background, the book divides itself geographically, and proceeds with an empirical section on Egypt and on the Arab Gulf. In both sections, a chronology of the construction of different courts is presented. Given these premises, however, something is lacking with the execution. While one could note, as the author does, some Egyptian influence across the legal system of the Arab world and use this point to unify the book, this does not quite fit with the material at hand, nor with the hypothesis of the preceding chapter. The hypothesis that the legal system is a part of the hegemony gets lost without a fuller development of what that hegemony is and what role law is postulated to play in that kind of hegemony. Assuming such a fuller development, however, it's not clear what kind of unity the book would have. Brown also seems to be a partisan of the idea of the autonomy of law, without being able to argue for its connection to either the Egyptian or Gulf forms of hegemony. The theory reader is thus left hanging, if one can use such phrases in the review of a law book.

In the Egyptian section, Brown, much indebted to earlier work by Byron Cannon and Farhat Ziadeh, pursues the account of the Egyptian legal system from 1876 onward. In that year, the Mixed Courts appeared. National Courts and their Codes appeared in 1883. The Codes were applied to Cairo and Lower Egypt in 1884 and to Upper Egypt at the end of the decade. Egyptian nationalist politicians hoped that the formation of this system of courts would allow Egypt gradually to take over the function of the Mixed Courts, which they despised. Curiously, during the heyday of the colonial period (1884–1922), the British courts collided with the Mixed Courts and came to regret their existence, according to Brown. Thereafter it was the Egyptian rulers who experienced frustrations with the court system. Thus it was that the establishment of an Appeals Court in Asyut, the capital of Upper Egypt, in 1926 threatened the centralization of the system, obliging the setting up of a Court of Cassation in Cairo as the apex to the system. Similar concerns for centralization were expressed concerning the decision to abolish the Shari'ah and confessional courts in 1955. While this subject has yet to be studied in detail, one might suggest that with much of the Christian population dwelling in Upper Egypt, the regional north-south issue again seems to be the real underlying factor in calls for centralization.

In the Gulf section of the book, Brown postulates an autonomous but dilatory legal system. His conclusion, following that of the Cambridge School line of historical thought, is that legal reform was not influenced so much by imperialism and liberalism as by the local needs of state-building. In the Gulf as in Egypt, Brown claims, rulers have wanted to and have been able to combine autocracy and rule by law. Yet pointing to the influence of the Egyptian legal authority 'Abd Al Razz'a al- in the Gulf countries, Brown seems persuaded of a kind of Arab regional legal culture. But would not such a view ultimately weaken the argument that rulers develop law as a part of their hegemonies, given that Egypt and the Gulf are obviously quite different? This issue is not confronted. In one of the final chapters, Brown does note that divorce issues in the Gulf would not show up in court as in Egypt but would be handled by the Shari'ah. For the Gulf, it is clearly gender issues are not only society but also politically sensitive. In sum, Brown's book suggests ways in which the field of Arab legal studies could go.

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JAMES L. GELVIN. *Divided Loyalties: Nationalism and Mass Politics in Syria at the Close of Empire*. Berkeley and Los Angeles: University of California Press. 1998. Pp. ix, 335. Cloth \$50.00, paper \$19.95.

The political elite of greater Syria faced an uncertain future when the Ottoman armies withdrew in 1918. British troops occupied what would become mandated Palestine, and the French controlled the coastal re-

gions to the north. In Damascus, the Amir Faysal, as nominal head of the putative "Arab Revolt," was torn between those from among Syria's traditional elite who pushed him to announce an independent Arab kingdom and his own desire not to offend his British patrons. Previous scholarship has focussed on that narrow elite, seeking to establish when they embraced the cause of Arab nationalism and Faysal as their sovereign. But while Arab nationalist hagiography holds that an Arab national consciousness was awakened in the waning decades of the nineteenth century and that the revolt in the Hijaz tapped into an already articulated national will, recent historical revisionism has suggested that the nationalist moment arrived much later. It has also been shown that not all Syrian elites embraced the Arab nationalist agenda with equal enthusiasm. The older members were more conservative than those younger in age; the embrace of Faysal's cause was more pronounced among those who had more recently nudged their way into the ranks of Syria's elite; there were wide regional and religious variations. Those caveats aside, most scholars have come to a consensus that, however late the moment of nationalist epiphany arrived, Syria's leading families threw their lot in with Faysal's Arab kingdom at the end of the war in opposition to British and French plans to divide the Fertile Crescent between them.

James L. Gelvin's study does not subvert that consensus as much as it questions whether the focus of the historical inquiry has been misplaced. Rather than concentrate solely on the elites, Gelvin seeks to explicate how elite concepts of the nation were translated into the language of the street. Employing an impressive array of archival sources as well as personal interviews, the author presents a lucid picture of the political chaos that marked the transition from Ottoman to French rule in Damascus between 1918 and 1920. Rather than one unified vision of "Arabness," he has identified many different visions of what an imagined political community might be, whether configured as the Arab kingdom or simply Syria.

Gelvin establishes two broad, and very different, sets of actors who competed to define the nation and win the hearts and minds of the Syrian people. In one camp were the intellectuals, long identified as the formulators of Arab nationalism. Scions of old and noble families, these styled themselves as the "enlightened" elite of Syria with a self-imagined destiny to lead the Syrian masses into a liberal, modern state with Faysal as its king. The Arab Clubs that sprang up throughout greater Syria, and that were supported by Faysal's government, gave their ambitions voice and attempted to inculcate the Syrian masses with the new vocabulary and symbols of their political identity. Their vision was not uncontested, however. As Gelvin draws out convincingly, opposition to rule by Faysal and his *Hijazis* emerged not only among those who thought that the leadership was too conservative religiously for multi-sectarian Syria but also in strata of the population that have previously been ignored in studies of the period.

These included religious scholars, merchants, craftsmen, and even, on occasion, city toughs and rural bandits. This diverse group managed to find its voice in the Popular Committees that were initially established to resist the French. Increasingly, however, the leadership of these committees found itself at odds with the Sharifian regime and in the waning days of Arab independence actually emerged in opposition, not only to the French but to Faysal.

Besides identifying the diversity of nationalist discourse in post-Ottoman Syria (no small feat), Gelvin explores how the various factions sought to mold public opinion to their respective causes through use of slogans, parades, theater, and street demonstrations. This section of the book is particularly instructive in helping to explain the origins of the "politics of the street" that have played such a prominent role in Arab political life in this century. Although Gelvin's book has a narrow time focus, his analysis of mass politics in that period is invaluable for anyone wishing to understand twentieth-century Arab history. With that century now at end and many sage observers predicting the end of the Arab nationalist moment in history, this book helps us to understand how it all began.

BRUCE MASTERS
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CYRUS GHANI. *Iran and the Rise of Reza Shah: From Qajar Collapse to Pahlavi Rule*. London: I. B. Tauris. 1998. Pp. xiv, 434. \$45.00.

Iranian historiography contains a large lacuna stretching from 1909 to 1925. Although historians have explored the previous as well as the subsequent eras—the constitutional revolution of 1905–1909 and the reign of Reza Shah (1925–1941)—they have on the whole shied away from the intervening period with its economic stagnation, incessant foreign interventions (including World War I), provincial revolts, and inconsequential struggle for power among court notables, landed families, tribal chiefs, and clerical dignitaries. What is more, these labyrinth years are seen as leading to nowhere—except, perhaps, to the 1921 coup carried out by Colonel Reza Khan, who soon crowned himself Reza Shah and founded the Pahlavi dynasty.

Cyrus Ghani's book is a valiant attempt to fill the gap for the 1919–1925 years. Using predominantly the archives of the British Foreign Office, supplemented with Iranian memoirs, Ghani has done an admirable job both in detailing London's policy toward Iran—especially Lord Curzon's controversial 1919 Anglo-Persian agreement—and in untangling the complicated maze of Teheran politics. These six years witnessed the constant rotation of ministers including nine premiers and nineteen cabinets.

Writing clearly and succinctly, Ghani has produced a history in the best traditional-Rankean style. This is very much a political "history from above." It relies heavily on British documents and Iranian memories,

especially those of government officials. It shuns theoretical speculations and searches for the "hard facts," for the past "as it was." It provides Namier-like biographies—even identifying, where possible, the Oxbridge colleges—of those who rotated in and out of the cabinet. What is more, it manages to provide a highly detailed account of British involvement in making and unmaking the cabinets as well as in trying to incorporate the country into the British sphere of influence, without using the taboo word "imperialism." This unseemly term appears only once in the entire book, and then solely in reference to Russia (p. 15). In short, this book will warm the hearts of many a traditional historian in the West.

Despite these assets, the book is very much a product of historiography in Iran rather than of Iran. For it is overly preoccupied with issues that have plagued writers in Iran but that leave the outside historian somewhat cold. It dwells on the question of whether Reza Shah was, or was not, helped to power by the British and therefore was, or was not, a British puppet. Obviously, such a question has political implications in modern Iran. Ghani comes to the conclusion that Reza Shah most certainly received support from the British to carry out his coup, but once in power he was in no way beholden to them. This may be a new finding for some, but it is more or less the same conclusion that historians outside Iran reached long ago. What the book does with much detective work is to show that the British support for the coup came not from Curzon and his Foreign Office, but from General Ironside, the commander of the British military in Iran. Not for the first time or for the last, the various British agencies in the Middle East—notably the Foreign Office, the War Office, and the India Office—were working at cross purposes and intentionally keeping each other in the dark.

Moreover, the book is constantly on the lookout for bad guys and good guys, freely passing judgments on the array of dignitaries that circulated on and off the political stage. Individual politicians are described as "venal," "unprincipled," "avaricious," "flawed," "lazy," "timid," "brave," "cruel," "self-seeking," "honest," "dishonest," "generous," "gentlemanly," "simple," "uneducated," "arrogant," "decent," "ambitious," "impaired," "wanting in substance," a "man of dignity," or a "man of no scruples." Such judgments are not only jarring and suspect, especially when reliant on British assessments, but they also distract from the more significant question of why the constitutional revolution heralded two decades of political instability, provincial revolts, and state disintegration. To address that question, one needs a deeper socioeconomic analysis, preferably a "history from below," rather than this type of "history from above." Thus the period 1909–1925 remains lacuna.

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SUB-SAHARAN AFRICA

ERIC SILLA. *People are not the Same: Leprosy and Identity in Twentieth-Century Mali*. (Social History of Africa.) Portsmouth: Heinemann. 1998. Pp. xi, 220. Cloth \$60.00, paper \$26.00.

Leprosy, or Hansen's disease, which has largely been eradicated in the industrial world, still afflicts thousands of people in less developed countries, including the West African nation of Mali. The disease has traditionally created a group of outcasts from society labelled as "lepers," who often formed communities isolated from the general population. In this book, Eric Silla focuses on leprosy and lepers in colonial and postcolonial Mali, especially among the Bamana people, with particular attention to the transformations in identity that occurred with the physical changes caused by the disease. While some studies exist on tuberculosis and sleeping sickness in Eastern and Southern Africa, very little has been done on the history of disease in West Africa or on leprosy. Silla's book makes an important contribution to this important and growing field of social history because of its focus on the changing social identities of people with the disease—who emerge from this study not as victims but as active participants in larger historical events. Their unique perspectives shed new light on some aspects of twentieth-century Malian history.

After a brief discussion of illness in Africa and the biology of leprosy, Silla presents a compelling life history narrative by a woman leprosy patient named Saran Keita who was in her early 70s in 1992. Her varied experiences reveal that leprosy was a central but not exclusive component of her multiple and ever-changing social identities. Saran Keita's life story consists of a series of events that changed her own perception of herself and the disease, eventually leading her to a leper community. Her biography also reflects some of the transformations that occurred in Malian society during the course of the twentieth century. This chapter is the book's strongest and most readable. Silla seeks to provide a patient-centered approach to the study of Hansen's disease and give voice to its sufferers. This chapter in particular admirably achieves those goals.

The author then briefly considers leprosy in precolonial Mali before analyzing how one developed a social identity as a leper in that rural agrarian society. As the disease progressed and became more noticeable, lepers were more and more isolated from daily life. The increasingly debilitating nature of the disease interfered with one's ability to marry, farm, and fulfill other everyday social obligations. When they began to seek treatment, usually from indigenous healers and then from French doctors, the lepers became patients, another variation in their social identity. Many had to leave their villages and migrate permanently to colonial centers, most notably the capital of Bamako, seeking treatment. Many turned to begging to support themselves, creating another new identity.

Silla then examines the role of the colonial administration in treating leprosy patients in centralized medical institutions where they formed a community that sometimes was at odds with doctors and nurses, both African and European. In 1945, a large group of residents in an institution near Bamako revolted against the workload and size of daily rations, among other grievances. The disturbance was quickly and harshly crushed, yet it also contributed to the sense of community among the patients. The author's extended discussion and criticism of colonial medical policies and practices is especially insightful. He cites an authoritarian approach to healing; a failure to acknowledge indigenous attitudes; a separation of leprosy treatments from all other health services; and severely limited investment in education and infrastructure.

Independence in 1960 did not mark a turning point in the medical history of Mali because colonial policies and practices, especially in regards to Hansen's disease, were perpetuated by the independent government of Modibo Keita and his successor, General Moussa Traore. Lepers continued to migrate to regional centers, and many became beggars despite government efforts to isolate the patients from the general population. Attempts to eradicate or contain the disease have largely failed, and leprosy continues to be a problem for the democratically elected government of Alpha Omar Konare even today. In recent years, however, the patients have formed associations that, while lacking any political power, do offer mutual support and a forum for socializing.

While more oral narratives and extended biographies of patients would have been most welcome, this book makes a solid contribution to numerous areas of study. It will interest Africanists in several disciplines, especially history and anthropology, as well as those engaged in the study of medicine, disease, public health, and colonial history.

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RALPH A. AUSTEN and JONATHAN DERRICK. *Middlemen of the Cameroons Rivers: The Duala and Their Hinterland, c. 1600-c. 1960*. (African Studies Series, number 96.) New York: Cambridge University Press. 1999. Pp. xii, 252. Cloth \$64.95, paper \$24.95.

The Duala were and are a tiny group of people (ca. 20,000 by 1900) living on the estuary of the Wouri in what is now the port city of Douala, the gateway to Cameroon. As middlemen between Europeans and Africans in the interior, their influence on the Cameroon hinterland has long been out of all proportion to their numbers and so has been the amount of scholarly attention they have attracted. Their role as middlemen, too, is the justification for this study, the first book wholly dedicated to their history. For this is a thematic history. Ralph A. Austen and Jonathan Der-

rick claim that the Duala are worthy of all this attention because they are the paradigm of the history of middlemen all over Africa. They began as commercial intermediaries between the hinterland and Europeans in precolonial times. Then they became intellectual "brokers" as they adopted many facets of the European ways of living and thinking, including Christianity, and adapted them to Cameroonian conditions. Many Duala became literate, and some of their leaders received an advanced education in Europe that led them to nationalistic politics at an earlier time than other Cameroonians, for whom some of the Duala leaders became role models. Certainly, their role in the nationalistic historiography of Cameroon has become a central one. Ultimately, according to the authors, Duala leaders became a paradigm of African elites in general, for they interpret these elites still as middlemen between the "global hierarchy" and their own countries; according to the authors, such leaders "appear incapable, even when they attain the kind of state power that always eluded the Duala elites, of creating a self sustaining basis for these positions" (p. 191). Hence it is worthwhile to study Duala history as a paradigmatic example of the historical growth of middlemen roles, which are still so crucial in present-day Africa. For this reviewer, however, though the total reduction of contemporary leadership roles in Africa to a middleman (or rather middle person) position is overly simplified and should be treated only as a rhetorical flourish that enhances unnecessarily the significance of this work. The book stands on its own merits.

The theme of middlemen structures discussion in successive chapters on Duala settlement on the coast and their place in the slave trade (1600–1830), their adventures during the "legitimate" trade era (c.1830–1884), their position during the German colonial era

(1884–1914), the heyday of French mandate rule (1940–1941), and their role in the era of decolonization (1941–1960). The presentation of this substantive history is admirably lucid and concise to the point that sets of tables summarize not only economic but also political developments, and in every chapter footnotes refer the reader to more detailed studies about the points discussed.

The main foci of the book are economic and intellectual rather than strictly political, as one might expect. It is an approach in part dictated by the available written sources, especially before 1840 and in part by the focus on the Duala quality of "middlemen," and it succeeds thanks to the experience of the authors. Nevertheless, the reader should constantly remain aware of the main argument of this book as it privileges one interpretation over others. Yet he or she still can use the work to construct alternative visions of the Duala past, for it is also an admirable work of reference, a guide into the abundant historical literature about the Duala. This reviewer only regrets one point. The historical experience of the Mpongwe middlemen on the Gabon estuary parallels that of the Duala from the 1600s onward so closely that a systematic comparison between the two would have both enriched and strengthened the main assertions made about the evolution of the coastal middleman position from brokerage in trade to the ultimate marginalization of leaders whose followers came to constitute tiny minorities in their countries and even in their own cities. Austen and Derrick chose not to pursue this parallel. Still, let us be grateful for their thorough history of the Duala which will not be superceded soon.

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Film Reviews

NOT FOR OURSELVES ALONE: THE STORY OF ELIZABETH CADY STANTON AND SUSAN B. ANTHONY. Directed by Ken Burns; produced by Ken Burns and Paul Barnes; written by Geoffrey C. Ward. 1999; color and black & white; 220 minutes. USA. English. Distributor: PBS Video.

I have never watched a Ken Burns film, until now. The thought of watching *The Civil War* (1990) or *Baseball* (1994) for eight or more hours seemed as appealing to me as housework. I read about Burns's documentaries and asked myself if I might not be missing something. Still, four hours (his shorter films) is a long time to sit in front of the television, and I could never quite make myself do it. That said, I must confess that I purchased *Not For Ourselves Alone* before I viewed it. Although I have never seen one of his films, I am well acquainted with the Burns mystique and was delighted that he had worked his documentary magic on one of my favorite topics in history: the nineteenth-century U.S. woman's rights movement. I had to own it. *Not For Ourselves Alone* is a good film, although it is no better and no worse than the earlier suffrage film *One Woman, One Vote* (1995), also broadcast on PBS (as part of the *American Experience* series).

As its subtitle states, *Not For Ourselves Alone* is "the story of Elizabeth Cady Stanton and Susan B. Anthony." Their lives and their friendship provide the lens through which the struggle for women's rights in the nineteenth and early twentieth-century United States is viewed. The film's opening sequence introduces the suffragists, "now old women," sharing their memories of the suffrage movement. The lives of these "very different" women, Stanton and Anthony, are then contrasted throughout the film. Born to wealth in 1815, Stanton married in 1840 and had seven children. Born five years later, Anthony was raised in a Quaker family, went to college, became a schoolteacher, and never married. The intelligent and upper-middle-class Stanton was considered the more radical of the two. Before meeting Stanton in 1851, Anthony worked in the temperance and abolitionist movements. After they joined forces, Stanton stayed at home to take care of her children and Anthony became her "arms and legs," conveying Stanton's rhetoric to crowds across America. Anthony's eventual focus on the single issue of

gaining the right to vote caused a split between the two women. Both died before the Nineteenth Amendment granting women the right to vote was passed in 1920, Stanton in 1902 and Anthony in 1906. The final sequence of this film highlights the ratification of the Nineteenth Amendment in Tennessee.

I have shown segments of this documentary to my history classes (at over three hours, it is too long to show the entire film), and half of the students in my women's history class moaned, "Susan B. Anthony, again?!" It seems that the women's history they learned in middle school and high school was of the famous woman variety. The rest of the class acknowledged that they had never heard of Anthony, and both groups had no idea who Stanton was. None of them had watched the PBS broadcast of *Not For Ourselves Alone* in November 1999. Although women's historians will find the content of this film familiar, it is quite clear that our students and the general public would probably agree with the *Mercury News* of San Jose, California, that Ken Burns has here tackled "a forgotten era of U.S. history."

Not For Ourselves Alone is a mixture of historic photographs and film clips, modern location footage, narration provided by Sally Kellerman, dramatic voice-overs delivered by a series of actors, and commentary supplied by female historians. Burns has made a point of stating that he could find thousands of historians to talk about the Civil War and hundreds to speak about baseball but no male historians to talk about Stanton, Anthony, or the woman's rights movement. He has also pointed out the "irony" that men produced, directed, and wrote the film, while women acted in supportive roles. Although troubling, these points cannot diminish the fact that one of the best features of the film is listening to Ellen Carol DuBois, Elisabeth Griffith, Judith Wellman, and other historians speaking about Stanton and Anthony. It is during these commentaries that the excitement, frustrations, and passion of the woman's movement come through. Unfortunately, most of the film is bland: not boring, but lacking controversy. I suspect that, if I had not already had an interest in the subject matter, I would have turned it off.

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SUNSHINE (*A Napfény íze*). Directed by István Szabó; produced by Robert Lantos and Andras Hamori; screenplay by Israel Horowitz and István Szabó. 1999; color; 180 minutes. Hungary/Germany/Canada/Austria; in English. Distributor: Alliance Atlantis Motion Picture Distribution, Toronto.

Sunshine, a rare film of sweeping ambition and creativity, is an epic chronicle of the fortunes of three generations of the Sonnenschein family, set against the shifting backdrop of Central Europe throughout the twentieth century. The story of this Hungarian Jewish family, whose fate is inevitably linked with world wars, revolutions, the Holocaust, and totalitarian regimes, is elegantly told by the narrator, Ivan Sonnenschein, the last surviving member of the family dynasty.

The story begins with Ivan's great-grandfather, Manó, the son of a publican who leaves his village at the age of twelve to make his fortune in the city, carrying in his pocket a book of recipes inherited from his father, which contains the secret ingredients of an herbal liqueur. The drink he concocts, "Taste of Sunshine," becomes widely popular, providing a stable foundation for the family's future. Manó's son, Ignatz, comes of age in a Hungary ruled by the Austro-Hungarian empire and enjoys a brilliant career as a powerful judge favored by the authorities under Franz Joseph's liberal monarchy. Ignatz balances his professional responsibilities with his love for his first cousin, Vali, whom he is determined to marry; the couple have two sons, István and Adam.

One of the two, Adam, becomes a world-class fencer who represents his country during the 1936 Olympics, but his triumph there carries little weight when anti-Semitic policies are introduced by the conquering Nazis. In the film's eloquent middle hour, Adam's victory as a "Christian" Olympic fencing gold medalist—Adam had converted to the country's dominant religion to further his fencing career—is stirring, while his shocking fate—captured by the Hungarian Nazi sympathizers of the Arrow Cross militia and dying in Auschwitz while defending his "Hungarian" honor—leaves the viewer caught in history's nightmare arc. Adam's son, Ivan, survives the death camps and makes his own career in the Stalinist era in Hungary, only to become part of the generation forced to come to terms with the nation's postwar Communist legacy.

Ralph Fiennes plays a different role in each generation of this resilient family: the grandfather, Ignatz, a practical lawyer; the father, Adam, the champion fencer; and the son, Ivan, with whom the story ends. Each is faced with a complex set of crises that threaten to betray the promise of the family name and bring about the loss of the Sonnenscheins' rich heritage. *Sunshine* is directed by Academy Award-winning master Hungarian filmmaker István Szabó, one of the leading directors of the postwar period. Over the course of his prolific career, has won over sixty major international awards; screened at the Twenty-Fourth Toronto International Film Festival and the Thirty-

First Hungarian Film Week in Budapest, *Sunshine* has already garnered additional plaudits, including awards from the European Film Academy (for best screenplay, cinematographer, and actor), the Academy of Canadian Cinema and Television (for best motion picture and achievement in sound editing), and the Foreign Critics' Gene Moskowitz Prize for best film shown at the Budapest festival. Celebrated for his cinematic meditations on Central Europe and its Habsburg history, Szabó's previous work includes *Mephisto* (1981) and *Colonel Redl* (1985), both of which investigate the relationship between morality and power.

Szabó co-wrote the screenplay for *Sunshine* with American screenwriter Israel Horowitz, foregrounding the history of patriotic Jews forced to assimilate, change their religion and their names (from Sonnenschein, meaning "sunshine," to Sors, which translates as "fate") in order to rise in Hungarian society as the country moved from Habsburg Empire through Nazism to the Stalinist era. According to the director, "This is not simply a question of ethnicity or race. From mobility among the various strata of society in recent Hungarian life to the waves of economic and political emigration in Europe today, people must face the challenge of surrendering the self or deliberately demonstrating their identity." The irony of a society that perpetually mourns the martyrs it creates and buries is perhaps the film's strongest point. Historic Budapest locations appear throughout the film, in sequences shot in the Opera House, the People's Stadium, the Museum of Applied Arts, the Synagogue in Dohány Street, the Parliament, the Western Railway Station, and in the city's streets.

What makes *Sunshine* such a moving experience is the intimacy of the stories and the vitality of the personalities explored. Szabó and his magnificent international cast transport us back through the events that shaped the past century as members of a Jewish family try to survive in a world undergoing massive changes. Yet this moving historical account never loses sight of the personal details that shape the legacy of an intergenerational family, transforming the tragedy of twentieth-century Jewry into a work of redemptive power.

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THE KNOT. Written and directed by Aleksandr Sokurov; produced by Svetlana Voloshina. 1998; 90 min.; color and black & white. Russian. Distributor: Film Studio Nadezhda, St. Petersburg, for ORT (Russian Public Television).

"Taisia Zakharovna Sherbak, daughter of a Kuban' peasant, a photograph of 1912. Isakii Semenovich Solzhenitsyn, from a peasant family, Stavropolskii Region, will volunteer for the Russian Army in World War I; awarded for bravery. These beautiful people,

Isakii and Taisia, will become husband and wife in 1917. Soon after, Isakii is killed. Their son Sasha, Sasha Solzhenitsyn, will appear in this world on eleventh of December, 1918, in the winter, a hard time of the year." With this narration, read by author-director Aleksandr Sokurov, we enter a short but expressively told life story of Nobel Laureate Aleksandr Solzhenitsyn. With solemn classical music in the background, performed by Solzhenitsyn's son Ignat, interlaced with the natural sounds of everyday life, we browse through old family photographs, almost hypnotized by Sokurov's slow, under-emphasized voice. "In 1941 he graduates . . . and leaves for the war . . . in 1942, while on the front, he writes his first stories . . . By the end of the war he is captain . . . In 1945 he is arrested . . . Since 1945, already for over fifty years, this man will have not one hour of peace. The stupidity of the rulers is a mortal disease. Silence of millions of witnesses, jealousy of the peers, exile, hard work."

Sokurov conveys his sadness that his Russian compatriots do not fully appreciate Solzhenitsyn. But one must readily agree with Sokurov that the Nobel Prize given Solzhenitsyn in 1970 made him "untouchable" by the regime. Sokurov quotes Solzhenitsyn's unforgettable words from the Nobel Address: "What can literature [do] against the assault of open coercion? Let us not forget that coercion does not live alone or is capable of living alone: it is necessarily interlaced with lies . . . And [it takes] a simple step of a simple brave man not to participate in lie: let this lie and coercion rule in the world, but not through me. The lie can withstand a lot in the world, but not against art. And as soon as the lie is refuted, the nakedness of coercion will be exposed in its ugliness."

Of his eighteen years of exile in Vermont, the writer says: "I was amazed in America by two aspects in nature. Completely different pine trees . . . And secondly, there are almost no singing birds . . . There are beautiful birds but they do not sing . . . There is fear of death in the West. Not just in the West, but all well-to-do people have a fear of death, which eclipses reason. If one can reach a condition in which one is not afraid of death, and moreover ready give oneself to God's will, then death is a natural transition from one condition to the other . . . just like our peasants always understood when they peacefully died . . . In Russia we used to have a feeling of repentance. Now it does not exist."

Sokurov, one of Russia's and the world's leading filmmakers, has produced a fine oeuvre of feature films, the latest of which, *Molokh* (1999), was honored for best screenplay last year at the Cannes Film Festival. Less known outside Russia is Sokurov's even greater body of documentary films, ranging in length from ten minutes (*Sonata for Hitler* [1979]) to over five hours (*Spiritual Voices* [1995]). His gallery of film portraits includes Dmitri Shostakovich, Feodor Shaliapin, Andrei Tarkovsky, and Boris Eltsyn. *The Knot*, released in the year of Solzhenitsyn's eightieth birthday, offers an homage to the celebrated citizen and

writer. The director does not ask controversial questions (the only real weakness of the film); rather, he anticipates and mostly agrees with his subject's positions. Sokurov aspires to touch the great man and capture his image forever, or at least for as long as celluloid will preserve it. He succeeds in this task brilliantly.

Film combines a variety of cinematic means: still and motion photography, black and white, sepia and toned-down color stock. Sokurov employed such a variety of means by necessity in his first feature film, *Lonely Voice of Man* (1978). This mixing of stocks and techniques has since become his trademark, one he employs with impressive results. Here, he develops new stylistic variations: for example, still photographs do not just fill the screen but appear, as if icons, in a frame of human hands.

Sokurov, an heir to Tarkovsky, goes even further than his mentor in using a slow rhythm in his films and long silent takes, so that the images seem to enter the heart of the viewer directly, bypassing the mind. His use of pause is as effective in documentary cinema as it is in his feature films. "God help you," says Sokurov to Natalia, the writer's wife, who replies: "Thank you," and, a full twelve seconds later, adds: "This help we feel."

Sokurov constantly and masterfully juxtaposes a few images and sound sources at a time. For example, in one striking episode, we see three grounds: a large grandfather clock with swinging pendulum in front of a large window with city traffic behind it. We hear the metronomic sound of the pendulum, the natural noises of the street, and Sokurov's voice telling us about the emigration of the writer and his enormous archive. As the camera rolls back, the clock, window, and street shrink in the middle of the screen, becoming surrounded by the two-story wall of the archive. My favorite three-and-a-half minute sequence features extreme close-ups of the writer: a foot; a frame-full of his hair; a frame-size "thinking hand"; a stack of hair, in which we slowly recognize mustache and beard covering silently moving lips; an eye behind thick glasses; the hand of the writer proofing a manuscript; the "thinking hand" again. Finally camera rolls back and out into the adjacent room. An orchestra of natural sounds accompanies the scene, with the writer's breathing as its soloist.

Sokurov has written the screenplay in a style both expressionistic and primitivist. Director, interviewer, and narrator, he truly earned the title listed in the credits: "author of the film." As a viewer, I can now gratefully echo Sokurov's pronouncement in the film: "I hear his voice, and see his eyes—and remember everything, now for my whole life."

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BUENA VISTA SOCIAL CLUB. Written and directed by Wim Wenders; produced by Deepak Nayar. 1999;

color; 104 minutes. English and Spanish with English subtitles. Distributor: Artisan Entertainment.

LÁGRIMAS NEGRAS. Directed by Sonia Herman Dolz; produced by Kees Ryninks. 1997; color; 75 minutes. Spanish. Distributor: Holland Film.

Cuba, a nation of eleven million inhabitants ninety miles off the coast of Florida, has played a variety of important roles on the world stage in the twentieth century. Cuba became a major actor on a number of international occasions, from the war of 1898 and the Good Neighbor Policy of 1933–1947 to the Cuban Missile Crisis of the 1960s and the East-West conflict of the Cold War, during which Cubans engaged in a series of collaborations and interventions in the Third World, from Angola to Nicaragua.

Although Cuban history has been wrought by social and political conflict, the nation's lasting influence might be found in another realm: music. As Cristóbal Díaz Ayala, the author of *Cuando salí de la Habana* (1999), reminds us, Cuba has exported and influenced music around the world since the late nineteenth century. Indeed, Cuban musical forms such as the *habanera*, the *bolero*, the *mambo*, and the *cha cha cha* have inspired individuals worldwide. With the onset of the revolution in 1959, large numbers of Cubans left the island taking these sounds with them. In the 1960s and 1970s, Cuban musicians such as Silvio Rodríguez and Pablo Milanés were in the forefront of the politically committed musical movement known as *Nuevo Canto*, or the New Song Movement, which influenced leftist movements throughout Latin America.

Since the collapse of the Soviet Union, Cuba has experienced a worsening social and economic crisis, which has spurred a reorganization of the Cuban socialist system. One of the key changes was the reopening of the tourist industry and the creation of joint ventures, companies owned and operated by foreigners in alliance with the Cuban state. This collaboration has also found its way into various cultural forums, as many foreigners team up with Cubans to produce CDs, films, and a host of other commodities. Two such collaborations, *Buena Vista Social Club* and *Lágrimas Negras*, offer windows onto Cuba's rich musical tradition, although with surprisingly few direct references to the country's political, social, and economic problems. These two films document changes in present-day Cuba and challenge viewers to forge a dialogue between music and history and between Cuba's past and future.

Both films are romantically idealistic, recalling a bygone era of Cuban music from the 1930s to the late 1950s, and both celebrate some of the dying emblems of the pre-Castro era, symbolized by the old American cars that appear constantly in the footage from Cuba. By focusing on talented but forgotten musicians of the pre-Castro era, rather than the more politically committed musicians of the 1960s or 1970s or the popular salsa musicians of the 1980s and 1990s (such as Los Van Van or Irakere), both films connect postcommu-

nist with pre-Castro Cuba. In so doing, these films inadvertently fuel the nostalgic feelings not only of many Americans and Cubans in the United States who remember the Havana of the 1950s but also of Cubans in Cuba who now reminisce about an era that no longer seems so bad. At the same time, both films appeal to the younger generations who have (until recently) been denied access to much of Cuba's music.

Buena Vista Social Club gained popular attention in the United States following on the heels of the successful CD of the same name. The German director Wim Wenders, who directed this film in close collaboration with American guitarist and Cuban music lover Ry Cooder, combines elements of the "band on the road" movie with tapings of live concerts, recording sessions, and personal interviews with members of the band. The film culminates in a concert at Carnegie Hall, and a roaming camera follows members of the band around New York City, registering the seemingly innocent comments of Cubans seeing New York for the first time.

The film is clearly the work of an enthusiast. It does not follow the classic documentary style, which attempts to enlighten or inform viewers following a given argument or theme. Interviews are relaxed and sympathetic, and they stay away from politics. Indeed, historians will have to fill in many of the details about Cuban music with assistance from other sources. Documentaries produced by the Instituto Cubano del Arte e Industria Cinematográficos in Havana, such as *De donde viene la Rumba* (1980) and Michael Dibb's BBC documentary, *Qué se toca en Cuba* (1985) are much clearer as educational tools, as is the three-part documentary series on the evolution of Latin music in the United States, *Routes of Rhythm* (1997).

Yet, it is precisely the "untouched" character of the music and musicians in this film that draws viewers in and raises a number of questions of interest to the historian. Two leading vocalists (Ibrahim Ferrer and Omara Portuondo, a 1950s Cuban radio personality) join together with a number of musicians, including guitarist Eliades Ochoa, trumpeter Manuel Vázquez, and pianist Rubén González, to perform a number of *sons*, *guajiras*, a *danzón*, and a few *boleros* with such grace that the viewer cannot help but be moved. The older generation in Cuba, the majority of them performers before the revolution, thrive despite the country's economic and political decay, seen as the camera roams around Havana and enters into the homes of some of the band members.

The failure of the Cuban Revolution, which aimed to eradicate class difference, sexism, and racism, is evident. For many of the musicians, it is as if the revolution never occurred. Ninety-year-old Compay Segundo, with his beautiful baritone voice, is a perfect symbol of the revolution: aging yet charming, romantic and seductive but unrealistic as he speaks of his love for Cuban music and his passion for women.

Romanticism aside, to understand the charm of this film, it is instructive to examine Ella Shohat's essay,

"Gender and the Culture of Empire: Towards a Feminist Ethnology of the Cinema" (1991). Shohat takes Hollywood films to task for a neocolonial pattern in which Western protagonists go into a foreign territory and uncover hidden secrets or find treasures hidden from the natives, using indigenous culture as a backdrop. While Cooder and Wenders certainly come off as heroes (for rediscovering Cuban treasures supposedly so hidden from ordinary Cubans that most *habaneros* had even forgotten where the Buena Vista Social Club used to be), the filmmakers do not use Cuban culture as mere backdrop. Rather, they integrate themselves into it, as Cooder and his son perform with the band. Cooder and Wenders have done some of the work of the historian by exposing these hidden treasures, although they fail to place the music in a historical or social context. The film lets the music and characters reveal themselves on their own terms, and for our enjoyment.

The lesser-known *Lágrimas Negras*, by Dutch filmmaker Sonia Herman Doltz, is another joint production based on a musical group, this one hailing from Cuba's second city, Santiago de Cuba. Doltz's camera creates a similar "band on the road" type of movie as she follows La Vieja Trova Santiaguera (The Old Santiago Troubadours), comprised of five men (Reinaldo Hierrezuelo, Reinaldo Creagh, Pancho Cobos, Aristoteles Limonta, and Ricardo Ortiz) between the ages of sixty-two and eighty-four. Doltz has edited and pulled together footage of interviews around Santiago and from the band's tour across Europe.

Released before *Buena Vista Social Club*, the film also followed on the heels of a successful CD, *La Vieja Trova Santiaguera* by the Spanish producer Manuel Dominguez, recorded for a Spanish label in 1994. The

film's title has many meanings. It is both sad and nostalgic, showcasing an Afro-Cuban musical group from a city considered the most Afro-Cuban, but one that is clearly undergoing severe economic decay. The song "*Lágrimas Negras*," however, is a 1930s *bolero* (a celebration despite sadness and loss) written by Miguel Matamoros, considered one of the founding fathers of Cuban popular music.

Doltz captures the feelings of sorrow and longing in addition to a *joie de vivre* in many of the concerts, performances, and interviews. At one point, Doltz juxtaposes local scenes of unbridled joy with frenetic applause at the concerts in London and Barcelona, going back and forth from local Cuban scenes to concert scenes abroad, marking the different ways that Cubans and Europeans celebrate but also emphasizing the universality of Cuban music. As in *Buena Vista Social Club*, the men's personal views (particularly on gender issues) are hopelessly out of step with many of the social values promoted by the revolution. Yet the band members all attest to the importance of the official revolution to their lives. In one scene (which does not seem contrived) on the road to London, the band visits and pays homage to the grave of Karl Marx.

Neither *Lágrimas Negras* nor *Buena Vista Social Club* place their subjects into the larger context of Cuban musical development. Nor do they ask major questions, such as: "Why or how have these musicians been forgotten?" Nonetheless, both films are testaments to the power of Cuban music in history and its rising influence today. It is not so much that a new generation is revisiting an old but that the old is back again.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

METHODS/THEORY

NICK MERRIMAN, editor. *Making Early Histories in Museums*. (Making Histories in Museums.) New York: Leicester University Press. 1999. Pp. xii, 212. \$89.50.

SUSAN M. PEARCE, Presenting Archaeology. BARBARA WOOD and JONATHAN COTTON, The Representation of Prehistory in Museums. MARY BEARD and JOHN HENDERSON, Rule(d) Britannia: Displaying Roman Britain in the Museum. SAM LUCY and CLARE HERRING, Viewing the "Dark Ages": The Portrayal of Early Anglo-Saxon Life and Material Culture in Museums. STEPHANIE MOSER, The Dilemma of Didactic Displays: Habitat Dioramas, Life-groups and Reconstructions of the Past. SIMON JAMES, Imag(in)ing the Past: The Politics and Practicalities of Reconstructions in the Museum Gallery. MARIE LOUISE STIG SØRENSEN, Archaeology, Gender and the Museum. ANGELA PICCINI, Wargames and Wendy Houses: Open-air Reconstructions of Prehistoric Life. JANET OWEN, Interaction or Tokenism? The Role of "Hands-on Activities" in Museum Archaeology Displays. ALAN SAVILLE, Thinking *Things* Over: Aspects of Contemporary Attitudes Towards Archaeology, Museums and Material Culture.

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ARTICLES

TO THE EDITOR:

Anniversaries celebrated in the recent past—the bicentennial of 1776 and the quincentennial of 1992—have served as occasions for extensive historical revision. In 1976, it was brought home to us that, contrary to the accepted wisdom, the United States of America and its history has not been free of national ideology, or ideologies, as simple a truth as that now seems. In 1992, what started out as a grand celebration of Columbus's discovery of America quickly turned to a condemnation of him as a destructive invader and a celebration of American cultural diversity—an interpretive somersault accompanied by calls for the dominant foundation story to be scrapped. As a result, American history is still at sea, not necessarily an unhealthy location. Yet the arrival of the current stellar date, the year 2000, seems to have aroused mostly apathy among historians of America and elsewhere. Those marking it have resurrected the question of the reception of the year 1000, or recapitulated the last thousand years, or the last century, or looked into the history of various millenarian movements. Where, though, are the deeper questions and considerations such as surfaced in 1976 and 1992?

Reflection has exposed some faultlines. Richard K. Emmerson in the December 1999 issue of the *AHR* [1603–14], has pointed to a lack of clarity in terminology associated with the key word millennium. That fuzziness, along with the limited range of subject matter covered in the *AHR Forum: Millenniums*, raises some far broader questions: Is this the best we can do

to approach the millennium historically? Should we be content to look at some instances of gendered approaches to the millennium or at a spate of millenarian movements? In passing, Emmerson cites Bernard McGinn's observation that "most medieval apocalyptic beliefs were employed as ecclesio-political rhetoric in the service of dominant institutions and officeholders (popes and other ecclesiastical leaders; emperors, kings, and their propagandists)" [1608]. He might well have gone on to say, as a solid bibliography evidences, that a major component in those apocalyptic beliefs, the millennial prospect, has loomed large in such mainstream, or establishment, political theology, and that the millennium has been invoked to support existing institutions and authorities far beyond those of the medieval West. At the same time, there is good evidence that millenarian movements may be understood as extreme expressions of certain culture-wide or sometimes cross-cultural ideational currents. Should we not then give some thought to the history of the broader scope of millennialism and its range of occurrences, its stories, language, and images?

The reach of "ecclesio-political rhetoric," of political theology wielded by people in power, has not been confined to an elite or restricted to theory or discourse. It has had wide historical impact. Why have nonspecialists paid it so little attention? In part, I would suggest it has to do with a far better acquaintance with millenarian movements. They are intrinsically more dramatic, and ready at hand are readable interpretations of them as responses to social injustice or forerunners of totalitarian regimes, explications that tend to preclude closer consideration of impelling beliefs. What gets shortchanged are the more problematic yet more central aspects of millennial history and its intimate ties to religion and religiosity.

It is inescapable that the concept of millennium itself and the triple-zero date now upon us stem from religious roots. We acknowledge that the advent of the year 2000 marks most literally the anniversary, as our calendrical system more or less computes it, of the birth of Jesus Christ, and that the notion of a millennium retains the biblical sense of a perfect number or time and also something of prophetic hopes and fears associated most strongly with the Book of Revelation. Yet we seem to have lost track of the intervening

filtering years, of the spectrum of millennial manifestations that until the eighteenth century were all at least religiously tinged.

To return, then, to what the marking of the current turn of the millennium may have exposed analogous to the revelations produced in the course of earlier commemorations in this country. During the anniversaries celebrated in 1976 and 1992, an interplay of current issues and historiographical reflection bared gaps, or anyway lags, in historical perception. In the year 2000, rising religiosity and adherence to organized religion in this country are contributing to a renewed questioning of where religion's public and private boundaries should be drawn. Religious resurgence is also challenging a received Western history that for several centuries has tended to celebrate the advance of secularism. At the same time, the turn of the millennium has revealed a penchant, among historians who do not specialize in them, to undervalue the ubiquity of religion and religiosity.

We have scrapped many of the old assumptions regarding the objectivity of historical truth. We have proclaimed *finis* to the idea of progress, circumscribed belief in the efficacy of reason, and, in the doing, largely relegated to history a two-centuries-long enchantment with and emphasis on secularity. What we have not yet done, what the attempt to comprehend the millennium in historical terms makes clear, and what a resurgence of religiosity in this country makes highly desirable, is give adequate weight to the reach and potency of religion and religiosity in the past. That does not mean that the millennium as commonly understood today necessarily carries religious meaning. But it continues to bear traces of its origins in sacred history that resonate yet. At 2000, millennial prophecy, belief, rhetoric, terminology, imagery, and their derivatives instance the scope and inclusivity of religion in the past, as well as remind us of the role religion has played in shaping the overall structure of the history we write.

PEGGY LISS
Washington, D.C.

Richard K. Emmerson does not wish to reply.

THE EDITORS

REVIEWS OF BOOKS

TO THE EDITOR:

Judith Laikin Elkin's review of my book, *Esau's Tears: Modern Anti-Semitism and the Rise of the Jews*, was published in the same issue of the *AHR* [104 (October 1999): 1370–71] as a rancorous exchange of letters (1448–49) relating to that book. I will not repeat here the points I made in the exchange, except to reemphasize that *Esau's Tears* has been widely and favorably

reviewed by respected scholars, as has my previous book, *The Jew Accused*.

Elkin begins by terming my book a "545-page polemic . . . [challenging] Jewish historiography from biblical times to the Holocaust." Many other reviewers have offered special praise for the volume's balance, its effort to look at issues openly and dispassionately. Elkin seems especially upset by what she perceives as my failure to recognize "a unique Jewish genius," by remarks of mine that she considers "antagonistic to the Jewish experience," and by my criticism of those historians who present anti-Semitism as exclusively the product of Gentile fantasies about Jews, unrelated to Jewish action in the real world. My main sin obviously is in not writing history "correctly."

Elkin's rising indignation at my interpretive impudence reaches a crescendo in her reference to my "most shameful thrust," that of granting equivalent moral status to Nazi leaders and concentration camp prisoners. This is one of the more preposterous of her many misreadings of my words, and I find it revealing that the very passage (on p. 510) that so outrages Elkin has recently been selected for special praise by Professor Robert Skloot, one of a number of respected scholars who have cited my books or assigned them to their classes. He especially applauds my "generous spirit" in dealing with these ineffably distressing matters, and he alludes at the same time to "holders of historical and cultural theories impervious to reassessment . . . [who lash out blindly when they] find their orthodoxy threatened" (*The Theater of the Holocaust*, 1999, 5).

In a stylistic sleight-of-hand that is indeed a "shameful thrust," Elkin laments that I criticize those extreme intentionalists "who refuse to relieve Adolf Hitler of the charge of murder"—as if I believe him innocent of that charge, when I in fact make a special point of his personal responsibility. She then goes on to fault *my* style as "illusionist" and "ambiguous." She accuses me of undertaking a "rehabilitation" of such historical figures as Richard Wagner, Wilhelm Marr, and Heinrich von Treitschke, when my clearly stated position is that these deeply flawed men need to be evaluated less polemically than has typically been the case. Even criminals have the right to a fair trial; I don't believe that historians should write with the mental horizons of a prosecuting attorney, or a lynch mob.

Elkin complains of an "amorphously defined Jewish 'rise'" in my book. I describe that rise precisely and concretely, reviewing its many-sided aspects, among them the acquisition of civil equality, unprecedented demographic expansion, in places spectacularly rising income, and, perhaps most impressively, intellectual influence and acclaim. This Jewish rise is of course central to the many histories that celebrate a "unique Jewish genius." In condemning my use of the notion of the rise of the Jews, Elkin completely omits reference to a key interpretive issue, what I term the interplay of fantasy and reality. I make an effort throughout the book to show how anti-Semitic fantasy interacted with

concrete issues having to do with a rising prominence of Jews in European and American life. I also point to a number of examples when a Jewish rise has not provoked widespread anti-Semitism. Contrary to the implications of her assertions, I do not "blame the Jews"—a facile slogan, at any rate.

In complaining that I have had frequent recourse to secondary sources, Elkin seems inadequately aware that the scholars I cite generally support the very interpretations that she finds so outrageous (e.g., in questioning the belief that the tsars encouraged pogroms); her apparent lack of familiarity with such areas of recent scholarly consensus may be explained by her own career-long specialization on one topic, the Jews of Latin America. Can Elkin possibly believe that a work of synthesis, ranging from Moses to Hitler and covering ten countries (none in Latin America) in the modern period, must be based mostly on primary sources? I should point out, however, that my discussion of the three men she mentions as being "rehabilitated" does in fact derive from research in primary sources, in German, as do my treatments of other important figures (Edouard Drumont, in French, Mussolini, in Italian, and Sholem Aleichem, in Yiddish).

Let me close by again noting that it is not easy to reply in a calm and fruitful manner, within the allotted 700 words, while wiping off the quantity of mud and muck hurled in my direction in this review and in the letters mentioned above.

ALBERT S. LINDEMANN
*University of California,
Santa Barbara*

TO THE EDITOR:

One does wonder what book Judith Laikin Elkin actually read in preparing her review of Albert S. Lindemann's book on European anti-Semitism [*AHR* 104 (October 1999): 1370–71]. It certainly was not the careful and innovative contribution of this fine scholar. I am afraid that Elkin's diatribe will only convince some that while many groups may legitimately be studied, warts and all, Jews are forever exempt because of the Holocaust. That constitutes another posthumous victory for the Third Reich.

NORMAN RAVITCH
*University of California,
Riverside*

TO THE EDITOR:

I am writing in response to Judith Elkin's review of Albert Lindemann's *Esau's Tears* in the October 1999 issue of the *American Historical Review* [1370–71]. I was at the University of California, Santa Barbara, for three years in the early 1990s and am presently completing my dissertation in U.S. history. I have worked closely with Albert Lindemann during my years at Santa Barbara. During the entire span of my extensive

interaction with him, I experienced not the slightest shred or hint of anti-Semitism from him. On the contrary, I found him to have, especially for a non-Jew, a rare and unusual empathy and understanding of Jews and the Jewish experience. My work with him focused extensively on the interrelated subjects of fascism and anti-Semitism. My impression was that it was because anti-Semitism was so foreign to Lindemann's own nature that he found its existence in others so puzzling. It seemed evident that his work in the area of anti-Semitism grew out of a sincere and empathetic desire to delve into the historical roots of its arising.

It is highly unfortunate that publication of his most recent book has led to Lindemann being subjected to ugly accusations of anti-Semitism. He is, indeed, very much the opposite of an anti-Semite. During all of my time at Santa Barbara, he was exceedingly helpful, friendly, and the source of much wise insight into history. Lindemann's book seems to me to be an effort to explore the interrelationship of major aspects of Jewish history, especially during the nineteenth and early twentieth centuries, to the sometimes parallel force of anti-Semitic currents. It does not make sense that these two areas of Jewish history and anti-Semitism must be separately compartmentalized rather than having their relationships examined. To seek to analyze roots of anti-Semitism is certainly not the same as seeking to excuse anti-Semitism, and especially its virulent and violent expressions, on these bases. I am certain that the latter was never his intent.

Lindemann's work seems to me to open important new ground in examination and discussion of such questions. These are, of course, subtle waters to navigate well. I believe it showed considerable courage, and willingness to expand beyond existing parameters in this field, for Albert Lindemann to venture into this potentially land mine-laden area. I can assure you, I believe, of his having only good intentions in his work.

WILLIAM GOLDSTEIN
Portland, Oregon

JUDITH ELKIN REPLIES:

First, to clear away some misconceptions. William Goldstein's defense of his mentor is unnecessary: in my review of *Esau's Tears*, I do not charge Albert Lindemann with anti-Semitism. What I wrote was that Lindemann's ambiguous formulation of historical events provides an insufficient basis for his thesis that Jacob's actions are the prime cause of Esau's tears. One who follows the page numbers that I cite to support my view could hardly disagree that the text is laced with ambiguities which cannot sustain the conclusions that Lindemann draws from them.

Norman Ravitch, who may be unfamiliar with textual analysis, calls my review a diatribe. It is my contention that scholarly challenges are as valid in the field of Jewish history as in any other. Writing about

anti-Semitism should not provoke an automatic charge that the author is anti-Semitic; yet the author who takes up anti-Semitism as his subject is not thereby rendered immune from criticism. And, please, can we not have a debate about Jewish history without the Holocaust getting dragged in by the heels?

I empathize with Lindemann's bad luck in that my review was published in the same issue of the *AHR* that carried a rancorous exchange of letters written by other historians. The placement of their letters was an editorial decision that had nothing to do with me, yet it obviously distorted Lindemann's perception of my review. A cooler rereading will persuade him that he was wrong to accuse me of slinging mud and muck at him. He may send his letter of apology to me privately.

Now to the substance of the author's complaints. Far from being especially upset, as Lindemann charges, because he fails to recognize a unique Jewish genius, I was performing the traditional function of a reviewer when I informed readers that the author was not writing Jewish hagiography. Moving Jewish history away from its ancestor-worshiping tradition is a major achievement of the present generation of historians, one in which I have played a part. As I state in my review, Lindemann discards oversimplification and develops a persuasive counterargument for the complexity and unforeseeability of events. He is particularly persuasive in his attempts to redefine the historical character of the villains of Jewish history. Such analysis can be a service to the practice of history and should be encouraged, not condemned; but it needs to be carried out with objectivity and keen attention to the evidence.

Despite objecting to my reference to *Esau's Tears* as a polemic, Lindemann himself opens the polemic with the book's title, with its implication that, as Jacob succeeded in life by cheating his brother, so Jews have risen throughout the ages by cheating non-Jews of their just rewards. The text is polemical throughout, as is illustrated by his account of Orthodox rabbis in Germany in 1884 who declined to extend the principle of universal love to those who do not abide by the seven Noahide Laws. "Presumably," Lindemann comments in a phrase that would do credit to John Chrysostom, "... Germany's large and growing population of non-believers, the millions of working-class socialists, for example, or those who could no longer accept the God of the Jews and the Christians, were not deserving of love" (pp. 150–51). The author forgets that earlier in the book (p. 49) he listed some of these laws, which prohibit idolatry, blasphemy, sexual immorality, murder, robbery, and eating the flesh of live animals, traits that might give Lindemann himself pause when choosing whom to embrace.

Lindemann alleges that I am inadequately aware of new research in European history because I cannot tolerate his questioning the belief that the tsars encouraged pogroms. But I did not write anything of the sort. I was, rather, bringing attention to the casuistry with which he presents so many of his conclusions, as he does in the passage referred to by denying a

concerted plan, or plot, to foment the riots (p. 67). My stated objection was that it would be harder to prove that tsarist governments had concerted any plan than that they condoned pogroms. As though echoing my own thoughts, Lindemann writes that Nicholas's government was "divided, confused, and incompetent" (p. 304).

Throughout the text (as on p. 292, among others), Lindemann downplays the role of organized religion in fanning religious hatred by transmuting and dumbing-down the result of centuries of anti-Jewish teachings into what he calls popular fantasies. This line of thought leads him to the conclusion, "The rise of Jews in modern times [is] the most fundamental cause of modern racial and political anti-Semitism" (p. 532). He does not explore why this rise should have aroused murderous intentions among non-Jews, many of whom were benefiting from it. Words such as envy or jealousy or just plain nastiness never darken the portrait of non-Jews, who are depicted as suffering from Jewish successes such as Emancipation. There are, of course, Americans who still regret Mr. Lincoln's ill-considered Proclamation.

Finally, I did not write, nor imply, that Lindemann is guilty of blaming the victim. He expresses genuine compassion for Jewish victims of bigotry in many places throughout the book. But his empathy for Jews who are victims is not matched by acceptance of Jews who are successful. It is as though the concept of Jew is only a pseudonym for victim, and as though that is the way it ought to be.

JUDITH LAIKIN ELKIN
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University of Michigan

TO THE EDITOR:

I have no serious objection to Hiroaki Kuromiya's review [*AHR* 104 (December 1999): 1796–97] of my translation of Elena Zubkova's book—there is no reason for one—but I want to raise one curious point. On what grounds can a reviewer complain of a translator's *divergence from a text that the reviewer has not seen*? I did not translate Zubkova's previously published book. I translated quite a different text, a new manuscript that she provided me, a manuscript not available, so far as I know, to the reviewer. If there are, as the reviewer suggests, "changes made from the original Russian edition," is it not natural to attribute them to the author as the author's proper prerogative?

HUGH RAGSDALE
University of Alabama

HIROAKI KUROMIYA REPLIES:

Hugh Ragsdale's letter answers the question I raised in my review: "It is not clear whether these [changes between the Russian and the English editions of the book] are authorial changes or the translator's." They

are authorial. I raised this issue because even though Ragsdale noted in the "Translator's Introduction" that the book, "Translated and edited by Hugh Ragsdale," is "the second (first English) edition of this study" (p. x), he did not identify the first edition. I understood "the first edition" to refer to Zubkova's book in Russian, *Obshchestvo i reformy, 1945–1964* (Society and Reforms, 1945–1964) (Moscow, 1993). In my review, I pointed out some important differences between this "Russian original" and the English edition and wondered who made the changes.

The translator has now disclosed in his letter to the editor that the original is in fact not the published Russian book but "quite a different text, a new manuscript that she [Zubkova] provided me [Ragsdale]." As Ragsdale suggests, this manuscript was not available to me. Nor was the existence of such a manuscript made clear in the book. Thus I had assumed in my review that the English edition was translated and edited by Ragsdale based on the published Russian original (incorporating some changes made by the author for the English edition). Hence the question in my review. I thank Ragsdale for clarifying the point that I raised. Other readers may do likewise.

HIROAKI KUROMIYA
Indiana University,
Bloomington

TO THE EDITOR:

I heartily agree with Robert J. Schneller, Jr.'s letter in the December 1999 issue of the *AHR* (1831–32). Although the term "new military history" is still used and much good work continues to appear under that heading, the novelty of the term has long since worn off among military historians who have been working in the field for any length of time. Military history

today is a "big tent" that encompasses more traditional "guns and boats" historians, those focusing on the interaction between diplomacy and military force, social military historians in the John Keegan tradition, and scholars comparing Western and non-Western military traditions—to name only a few of the approaches. Indeed, some debate exists as to what "military history" really is, between those who see it as focusing on military institutions and others who would include subjects falling under the overall heading of "war and society." In short, a lot has happened since the "new military history" came into vogue.

It would be most enlightening for military historians as well as those outside the field, to have a senior military historian review the state of military history in the pages of the *AHR*. There is no shortage of candidates; Dennis Showalter, Russ Weigley, John Lynn, Richard Kohn, or Edward M. Coffman are only a few of those well qualified to comment on this subject. I hope that you will give Schneller's letter every consideration as an idea whose time has come.

DAVID W. HOGAN, JR.

U.S. Army Center of Military History

ERRATA

In the February 2000 issue, the name of the director of the film *Brother, Can You Spare a Billion? The Story of Jesse H. Jones* was misspelled (Table of Contents, p. x, review, p. 326, and Index, p. 350). His name should have been listed as Eric Stange. The editors regret the error.

The listings for president and president-elect were reversed on page 1(a) after the Index in the February 2000 issue. Eric Foner is AHA president for 2000, and Wm. Roger Louis is president-elect.

Index to *American Historical Review*, April 2000

The titles of articles and films in the *AHR* are printed in italics, and titles of books reviewed are in quotation marks. Books of collected essays are designated by (E). The reviewer of a book or film is designated by (R), the author of a letter for the Communications section by (C).

- Abbott, Carl (R), 557
"Abelard," by Clanchy, 600
Abels, Richard, "Alfred the Great: War, Kingship and Culture in Anglo-Saxon England," 595
Abrahamian, Ervand (R), 652
"Active Radio," by Land, 566
Alapuro, Risto, "Suomen synty: Paikallisena ilmiönä 1890–1933 [The Birth of Finland as a Local Phenomenon, 1890–1933]," 626
Albert, Peter J., Frederick E. Hoxie, and Ronald Hoffman, editors, "Native Americans and the Early Republic" (E), 662
"Alexander Hamilton, American," by Brookhiser, 539
"Alfred the Great," by Abels, 595
Ambler, R. W. (R), 611
"American Indian Population Recovery in the Twentieth Century," by Shoemaker, 567
"The Americanization of West Virginia," by Hennen, 558
"Anatomy of Mistrust," by Larson, 522
Anderson, Virginia DeJohn (R), 534
Arbena, Joseph L. (R), 590
Archibald, Robert R. (R), 511
Armstrong, Warren B., "For Courageous Fighting and Confident Dying: Union Champlains in the Civil War," 548
"Asylum for Mankind," by Baseler, 534
"At the Edge of Empire," by Barrett, 644
Atkins, G. Pope, and Larman C. Wilson, "The Dominican Republic and the United States: From Imperialism to Transnationalism," 584
Attie, Jeanie, "Patriotic Toil: Northern Women and the American Civil War," 547
"Auf der Suche nach der 'wahren Republik,'" by Mollenhauer, 624
Augusteijn, Joost, editor, "Ireland in the 1930s: New Perspectives" (E), 663
Austen, Ralph A., and Jonathan Derrick, "Middlemen of the Cameroons Rivers: The Duala and Their Hinterland, c. 1600–c. 1960," 653
"Bad Language, Naked Ladies, and Other Threats to the Nation," by Rubenstein, 588
Bailey, Peter (R), 614
Balińska, Marta A., "For the Good of Humanity: Ludwik Rajchman, Medical Statesman," 642
Bannister, Robert C. (R), 553
Bardaglio, Peter W. (R), 554
Barrett, Thomas M., "At the Edge of Empire: The Terek Cossacks and the North Caucasus Frontier, 1700–1860," 644
Barton, Michael (R), 548
"Based on a True Story," edited by Stevens, 515
Baseler, Marilyn C., "'Asylum for Mankind': America, 1607–1800," 534
"Battle for the Soul," by Widder, 543
Baylis, John (R), 606
"Bazaar India," by Yang, 529
Beckett, J. V. (R), 612
Bednarowski, Mary Farrell (R), 551
Benjamin, Thomas, *A Time of Reconquest: History, the Maya Revival, and the Zapatista Rebellion in Chiapas*, 417–50
Benn, Carl, "The Iroquois in the War of 1812," 542
Bentley, Amy, "Eating for Victory: Food Rationing and the Politics of Domesticity," 563
"Betrayal of the Innocents," by Mitchell, 617
"Betty Friedan and the Making of the Feminine Mystique," by Horowitz, 574
Bewley, Robert, John Coles, and Paul Mellars, editors, "World Prehistory: Studies in Memory of Grahame Clark" (E), 660
"Beyond Pluralism," edited by Katkin, Landsman, and Tyree, 569
"Beyond the Household," by Kierner, 538
The Big Picture, by Kolchin, 467–71
"Bismarck und seine Gegner," by Goldberg, 633
"Black Lung," by Derickson, 573
Blocker, Jack S., Jr. (R), 551
Bracken, Christopher (R), 533
Braslow, Joel T. (R), 518
"Bread, or Bullets! Urban Labor and Spanish Colonialism in Cuba, 1850–1898," by Casanovas, 583
Breen, Richard, Anthony F. Heath, and Christopher T. Whelan, editors, "Ireland North and South: Perspectives from Social Science" (E), 663
Bridges, Amy, "Morning Glories: Municipal Reform in the Southwest," 557
"Britain, Southeast Asia and the Onset of the Cold War, 1945–1950," by Tarling, 521
Britten, Thomas A. (R), 567
Brookhiser, Richard, "Alexander Hamilton, American," 539
Brophy, James M., "Capitalism, Politics, and Railroads in Prussia, 1830–1870," 631
Brown, Jennifer S. H. (R), 543

- Brown, Michael K., "Race, Money, and the American Welfare State," 570
- Brown, Nathan J., "The Rule of Law in the Arab World: Courts in Egypt and the Gulf," 650
- Brownell, Susan (R), 523
- Browning, Robert M., Jr. (R), 549
- Brudny, Yitzhak M., "Reinventing Russia: Russian Nationalism and the Soviet State, 1953–1991," 648
- Bryson, Anna, "From Courtesy to Civility: Changing Codes of Conduct in Early Modern England," 608
- Buena Vista Social Club*, directed by Wenders, reviewed by Davis, 657
- Burnham, John C., "How the Idea of Profession Changed the Writing of Medical History," 510
- Burns, Ken, 655
- Burrows, Edwin G., and Mike Wallace, "Gotham: A History of New York City to 1898," 536
- "The Business of Charity," by Sander, 550
- Bynum, W. F. (R), 518
- Cadigan, Sean T. (R), 530
- Cahn, Anne Hessing, "Killing Detente: The Right Attacks the CIA," 578
- Cameron, Jamie, "James V: The Personal Rule 1528–1542," 607
- Campbell, Carl C., "The Young Colonials: A Social History of Education in Trinidad and Tobago, 1834–1939," 585
- Cannadine, David, "The Rise and Fall of Class in Britain," 612
- Cantor, Norman F. (R), 600
- "Capitalism, Politics, and Railroads in Prussia, 1830–1870," by Brophy, 631
- "The Car and British Society," by O'Connell, 614
- Carlin, Martha, and Joel T. Rosenthal, editors, "Food and Eating in Medieval Europe" (E), 662
- Carmichael, Calum (R), 593
- Carpenter, Kirsty, and Philip Mansel, editors, "The French Émigrés in Europe and the Struggle against Revolution, 1789–1814" (E), 663
- Cartwright, Nancy, *et al.*, "Otto Neurath: Philosophy between Science and Politics," 637
- Casanovas, Joan, "Bread, or Bullets! Urban Labor and Spanish Colonialism in Cuba, 1850–1898," 583
- "Il caso Pardo Roques," by Forti, 640
- "Catholicism, Political Culture, and the Countryside," by Heilbrunner, 636
- "Caught between Roosevelt and Stalin," by Dunn, 564
- Cecelski, David S., and Timothy B. Tyson, editors, "Democracy Betrayed: The Wilmington Race Riot of 1898 and Its Legacy," 554
- "The Chan's Great Continent," by Spence, 523
- "China's Military Faces the Future," edited by Lilley and Shambaugh (E), 661
- Christelow, Allan (R), 516
- Clanchy, M. T., "Abelard: A Medieval Life," 600
- Clark, Andrew F. (R), 653
- Clark, Claudia (R), 573
- "Class and Cultures in England, 1918–1951," by McKibbin, 613
- Classen, Constance, "The Color of Angels: Cosmology, Gender and the Aesthetic Imagination," 518
- Clausen, Soren (R), 523
- "Close Encounters of Empire," edited by Joseph, LeGrand, and Salvatore, 513
- Cocks, Geoffrey (R), 634
- Cohen, Thomas M., "The Fire of Tongues: Antônio Vieira and the Missionary Church in Brazil and Portugal," 592
- Coles, John, Robert Bewley, and Paul Mellars, editors, "World Prehistory: Studies in Memory of Grahame Clark" (E), 660
- "The Collective and the Individual in Russia," by Kharkhordin, 646
- "Colonizing Bodies," by Kelm, 533
- "The Color of Angels," by Classen, 518
- "Colorblind Injustice," by Kousser, 572
- Conn, Steven, "Museums and American Intellectual Life, 1876–1926," 553
- Connolly, James J., "The Triumph of Ethnic Progressivism: Urban Political Culture in Boston, 1900–1925," 556
- Conrad, James H., and Thad Sitton, "Nameless Towns: Texas Sawmill Communities, 1880–1942," 558
- Cooper, Patricia A. (R), 550
- Copley, Antony, "Religions in Conflict: Ideology, Cultural Contact and Conversion in Late-Colonial India," 527
- Coryell, Janet L. (R), 538
- "Creating Beauty to Cure the Soul," by Gilman, 518
- "Creating Peace in Sri Lanka," edited by Rotberg (E), 661
- "Creating Socialist Women in Japan," by Mackie, 525
- "Crescendo of the Virtuoso," by Metzner, 622
- Crick, Bernard (R), 615
- Crosby, Travis L. (R), 610
- Cushing, Kathleen, "Papacy and Law in the Gregorian Revolution: The Canonistic Work of Anselm of Lucca," 599
- "Dal Sacco di Roma all'Inquisizione," by Firpo, 639
- Daly, Jonathan W. (R), 645
- Davies, R. W., "The Industrialisation of Soviet Russia"; Volume 4, "Crisis and Progress in the Soviet Economy, 1931–1933," 647
- Davis, Darien J. (R), 582, 657
- Davis, David Brion, *Looking at Slavery from Broader Perspectives*, 452–66
- de la Motte, Dean, and Jeannene Przyblyski, editors, "Making the News: Modernity and the Mass Press in Nineteenth-Century France," 623
- De La Pedraja, René, "Oil and Coffee: Latin American Merchant Shipping from the Imperial Era to the 1950s," 580
- "The Death of the KPD," by Major, 639
- "Death or Glory," by Edgerton, 604
- "Democracy Betrayed," edited by Cecelski and Tyson, 554
- den Otter, A. A. (R), 532
- Derickson, Alan, "Black Lung: Anatomy of a Public Health Disaster," 573
- Derrick, Jonathan, and Ralph A. Austen, "Middlemen of the Cameroons Rivers: The Duala and Their Hinterland, c. 1600–c. 1960," 653
- "Designs against Charleston," edited by Pearson, 546
- "A Desired Past," by Rupp, 575
- "Distilling Democracy," by Zimmerman, 551
- "Divided Loyalties," by Gelvin, 651
- Dolz, Sonia Herman, 657

- Domańska, Ewa (R), 510
 "The Dominican Republic and the United States," by Atkins and Wilson, 584
 Dunlavy, Colleen A. (R), 631
 Dunn, Dennis J., "Caught between Roosevelt and Stalin: America's Ambassadors to Moscow," 564
 Dunn, Durwood (R), 562
- "Eating for Victory," by Bentley, 563
 Edel, Andreas, "Der Kaiser und Kurpfalz: Eine Studie zu den Grundelementen politischen Handelns bei Maximilian II. (1564–1576)," 628
 Edgerton, Robert B., "Death or Glory: The Legacy of the Crimean War," 604
 Egnal, Marc, "New World Economies: The Growth of the Thirteen Colonies and Early Canada," 530
 Eksteins, Modris (R), 604
 Elkin, Judith Laikin (C), 678
 Engerman, David C., *Modernization from the Other Shore: American Observers and the Costs of Soviet Economic Development*, 383–416
 Engerman, Stanley L., *Slavery at Different Times and Places*, 480–84
 Erdmans, Mary Patrice, "Opposite Poles: Immigrants and Ethnicity in Polish Chicago, 1976–1990," 579
 "Eriarvoisuus, valistuksen lupaus ja rasismi," edited by Isaksson and Jokisalo (E), 664
 Erie, Steven P. (R), 556
 "European Culture in the Great War," edited by Roshwald and Stites, 604
 "Excessive Expectations," by Gwyn, 531
- Faderman, Lillian, "To Believe in Women: What Lesbians Have Done for America—A History," 575
 Farber, Paul Lawrence (R), 612
 "A Fatal Friendship," by Rogow, 539
 "Fearful Hope," edited by Kleinhenz and LeMoine (E), 660
 "Federal Britain," by Kendle, 615
 Felix, David (R), 559
 Fichtner, Paula Sutter (R), 628
 Field, Bruce E., "Harvest of Dissent: The National Farmers Union and the Early Cold War," 565
 "Fighting Slavery in the Caribbean," by Martínez-Fernández, 582
 "The Final Argument," by Kagay and Villalon, 595
 Findlay, John M., and Richard White, editors, "Power and Place in the North American West" (E), 662
 "The Fire of Tongues," by Cohen, 592
 Firpo, Massimo, "Dal Sacco di Roma all'Inquisizione: Studi su Juan de Valdés e la Riforma italiana," 639
 Fischer, Gayle V. (R), 655
 Fones-Wolf, Ken (R), 560
 "Food and Eating in Medieval Europe," edited by Carlin and Rosenthal (E), 662
 "Food in Global History," edited by Grew (E), 661
 "For Courageous Fighting and Confident Dying," by Armstrong, 548
 "For the Good of Humanity," by Balińska, 642
 Forestell, Nancy M., Kathryn McPherson, and Cecilia Morgan, editors, "Gendered Pasts: Historical Essays in Femininity and Masculinity in Canada" (E), 661
- Forey, A. J. (R), 596
 "Forged Consensus," by Hart, 562
 Formisano, Ronald P. (R), 545
 Forti, Carla, "Il caso Pardo Roques: Un eccidio del 1944 tra memoria e oblio," 640
 Fowler-Salamini, Heather (R), 586
 Fox, Daniel M. (R), 510
 France, John, "Western Warfare in the Age of the Crusades 1000–1300," 596
 Frantzen, Allen J. (R), 598
 Franz, Heike, "Zwischen Markt und Profession: Betriebswirte in Deutschland im Spannungsfeld von Bildungs- und Wirtschaftsbürgertum (1900–1945)," 634
 "The French Émigrés in Europe and the Struggle against Revolution, 1789–1814," edited by Carpenter and Mansel (E), 663
 "From Courtesy to Civility," by Bryson, 608
 "From Swords to Sorrow," by Jansson, 625
 Furgol, Edward M. (R), 607
- Gallant, Thomas W., *Honor, Masculinity, and Ritual Knife Fighting in Nineteenth-Century Greece*, 359–82
 "Gangutskaia Bataliia 1714 goda," by Krotov, 644
 Garrard-Burnett, Virginia, "Protestantism in Guatemala: Living in the New Jerusalem," 589
 Gates, Barbara T., "Kindred Nature: Victorian and Edwardian Women Embrace the Living World," 612
 Gelvin, James L., "Divided Loyalties: Nationalism and Mass Politics in Syria at the Close of Empire," 651
 "Gendered Pasts," edited by McPherson, Morgan, and Forestell (E), 661
 "Gentlemen and Freeholders," by Kolp, 539
 "Geschiedenis van de Zeeuwse landbouw circa 1600–1910," by Priester, 627
 Geyer, Martin H., "Verkehrte Welt: Revolution, Inflation and Modern München 1914–1924," 635
 Ghani, Cyrus, "Iran and the Rise of Reza Shah: From Qajar Collapse to Pahlavi Rule," 652
 "Ghosts in the Middle Ages," by Schmitt, 598
 "Giant of the 'Grand Siècle,'" by Lynn, 620
 Gill, Gillian, "Mary Baker Eddy," 551
 Gilman, Sander, "Creating Beauty to Cure the Soul: Race and Psychology in the Shaping of Aesthetic Surgery," 518
 Gilmore, David D. (R), 617
 Given, James (R), 595
 "Gladstone": Volume 2, "1865–1898," by Shannon, 610
 Goldberg, Hans-Peter, "Bismarck und seine Gegner: Die politische Rhetorik im kaiserlichen Reichstag," 633
 Goldstein, William (C), 678
 Goldstone, Jack A., *Whose Measure of Reality?* 501–08
 Goodich, Michael (R), 597
 "Gotham," by Burrows and Wallace, 536
 "Gout," by Porter and Rousseau, 609
 Graham, John T., "Theory of History in Ortega y Gasset: 'The Dawn of Historical Reason,'" 510
 Graham, Michael F. (R), 608
 Gran, Peter (R), 650
The Great Explanandum, by Hart, 486–93
 Grew, Raymond, editor, "Food in Global History" (E), 661
 Grill, Johnpeter Horst (R), 636

- Gruen, Erich S., "Heritage and Hellenism: The Reinvention of Jewish Tradition," 593
- Gwyn, Julian, "Excessive Expectations: Maritime Commerce and the Economic Development of Nova Scotia, 1740–1870," 531
- Hall, Kermit L. (R), 519
- Haller, John S., Jr. (R), 552
- Halliday, Terence C., and Lucien Karpik, editors, "Lawyers and the Rise of Western Political Liberalism: Europe and North America from the Eighteenth to Twentieth Centuries," 519
- Halsall, Guy, editor, "Violence and Society in the Early Medieval West," 595
- Hamer, David, "History in Urban Places: The Historic Districts of the United States," 512
- Hammack, David C. (R), 536
- Harmon, Alexandra, "Indians in the Making: Ethnic Relations and Indian Identities around Puget Sound," 544
- Hart, David M., "Forged Consensus: Science, Technology, and Economic Policy in the United States, 1921–1953," 562
- Hart, Roger, *The Great Explanandum*, 486–93
- Hartmann, Susan M. (R), 563
- "Harvest of Dissent," by Field, 565
- Haydu, Jeffrey, "Making American Industry Safe for Democracy: Comparative Perspectives on the State and Employee Representation in the Era of World War II," 560
- Heaman, E. A., "The Inglorious Arts of Peace: Exhibitions in Canadian Society during the Nineteenth Century," 532
- Heath, Anthony F., Richard Breen, and Christopher T. Whelan, editors, "Ireland North and South: Perspectives from Social Science" (E), 663
- Heilbron, J. L. (R), 638
- Heilbrunner, Oded, "Catholicism, Political Culture, and the Countryside: A Social History of the Nazi Party in South Germany," 636
- "Heisenberg and the Nazi Atomic Bomb Project," by Rose, 638
- Hellie, Richard (R), 644
- Henderson, Timothy J., "The Worm in the Wheat: Rosalie Evans and Agrarian Struggle in the Puebla-Tlaxcala Valley of Mexico, 1906–1927," 586
- Hennen, John C., "The Americanization of West Virginia: Creating a Modern Industrial State, 1916–1925," 558
- "Heritage and Hellenism," by Gruen, 593
- "Herrschaft in der Herrschaft," by Hohkamp, 630
- "History in Urban Places," by Hamer, 512
- "History of the Inca Realm," by Rostworowski de Diez Canseco, 591
- Hoffman, Ronald, Frederick E. Hoxie, and Peter J. Albert, editors, "Native Americans and the Early Republic" (E), 662
- Hofstra, Warren R. (R), 539
- Hogan, David W., Jr. (C), 680
- Hohkamp, Michaela, "Herrschaft in der Herrschaft: Die vorderösterreichische Obervogtei Triberg von 1737 bis 1780," 630
- Holden, Robert H. (R), 581
- "Holding Aloft the Banner of Ethiopia," by James, 555
- Hollinger, David A. (R), 637
- Honor, Masculinity, and Ritual Knife Fighting in Nineteenth-Century Greece*, by Gallant, 359–82
- Horowitz, Daniel, "Betty Friedan and the Making of the Feminine Mystique: The American Left, the Cold War, and Modern Feminism," 574
- "How the Idea of Profession Changed the Writing of Medical History," by Burnham, 510
- Howard, Philip A. (R), 583
- Howe, James, "A People Who Would Not Kneel: Panama, the United States and the San Blas Kuna," 590
- Hoxie, Frederick E., Ronald Hoffman, and Peter J. Albert, editors, "Native Americans and the Early Republic" (E), 662
- Hudson, Benjamin (R), 602
- Huggins, Martha K., "Political Policing: The United States and Latin America," 581
- Huppert, George, "The Style of Paris: Renaissance Origins of the French Enlightenment," 619
- Hurt, R. Douglas (R), 565
- Hutchinson, John F. (R), 642
- "Im Zeichen der Krise," edited by Lehmann and Trepp (E), 663
- "Imbalance of Powers," by Silverstein, 577
- "The Impossible Triangle," by Spenser, 520
- "Indians in the Making," by Harmon, 544
- "The Industrialisation of Soviet Russia"; Volume 4, "Crisis and Progress in the Soviet Economy, 1931–1933," by Davies, 647
- "The Inglorious Arts of Peace," by Heaman, 532
- "Iran and the Rise of Reza Shah," by Ghani, 652
- "Ireland and Her Neighbours in the Seventh Century," by Richter, 594
- "Ireland in the 1930s," edited by Augusteijn (E), 663
- "Ireland North and South," edited by Heath, Breen, and Whelan (E), 663
- "The Iroquois in the War of 1812," by Benn, 542
- Isaksson, Pekka, and Jouko Jokisalo, editors, "Eriarvoisuus, valistuksen lupaus ja rasismi" (E), 664
- Jabour, Anya, "Marriage in the Early Republic: Elizabeth and William Wirt and the Companionate Ideal," 541
- Jackson, James H., Jr., "Migration and Urbanization in the Ruhr Valley 1821–1914," 632
- Jacob, Margaret C., *Thinking Unfashionable Thoughts, Asking Unfashionable Questions*, 494–500
- Jacobson, Matthew Frye (R), 573
- Jaenen, Cornelius J. (R), 542
- "James V," by Cameron, 607
- James, Winston, "Holding Aloft the Banner of Ethiopia: Caribbean Radicalism in Early Twentieth-Century America," 555
- Jansson, Arne, "From Swords to Sorrow: Homicide and Suicide in Early Modern Stockholm," 625
- "The Japanese Occupation of Malaya," by Kratoska, 526
- Jeffreys-Jones, Rhodri (R), 578
- Jokisalo, Jouko, and Pekka Isaksson, editors, "Eriarvoisuus, valistuksen lupaus ja rasismi" (E), 664
- Jordan, David P. (R), 622
- Jordan, Winthrop D. (R), 546

- Joseph, Gilbert M., Catherine C. LeGrand, and Ricardo D. Salvatore, editors, "Close Encounters of Empire: Writing the Cultural History of U.S.-Latin American Relations," 513
 "Joseph Leidy," by Warren, 552
 Josephson, Paul R. (R), 649
 Judd, Richard W. (R), 558
- Kagay, Donald J., and L. J. Villalon, "The Final Argument: The Imprint of Violence on Society in Late Medieval and Early Modern Europe," 595
 "Der Kaiser und Kurpfalz," by Edel, 628
 Kaplan, Lawrence S., "The Long Entanglement: NATO's First Fifty Years," 606
 Karpik, Lucien, and Terence C. Halliday, editors, "Lawyers and the Rise of Western Political Liberalism: Europe and North America from the Eighteenth to Twentieth Centuries," 519
 Katkin, Wendy F., Ned Landsman, and Andrea Tyree, editors, "Beyond Pluralism: The Conception of Groups and Group Identities in America," 569
 Katz, Michael B. (R), 570
 Kauppi, Niilo (R), 626
 Kelley, Ninette, and Michael Trebilcock, "The Making of the Mosaic: A History of Canadian Immigration Policy," 532
 Kelm, Mary-Ellen, "Colonizing Bodies: Aboriginal Health and Healing in British Columbia, 1900-50," 533
 Kemp-Welch, A., editor and translator, "Stalinism in Poland, 1944-1956: Selected Papers from the Fifth World Congress of Central and East European Studies, Warsaw, 1995" (E), 664
 Kende, John, "Federal Britain: A History," 615
 Kent, John (R), 521
 Kerr, Ian J. (R), 529
 Kharkhordin, Oleg, "The Collective and the Individual in Russia: A Study of Practices," 646
 Kierner, Cynthia A., "Beyond the Household: Women's Place in the Early South, 1700-1835," 538
 "Killing Detente," by Cahn, 578
 "Kindred Nature," by Gates, 612
 "The Kingdom of León-Castilla under King Alfonso VII 1126-1157," by Reilly, 601
 Kingston-Mann, Esther (R), 643
 Kleinhenz, Christopher, and Fannie J. LeMoine, editors, "Fearful Hope: Approaching the New Millennium" (E), 660
The Knot, directed by Sokurov, reviewed by Soifer, 656
 Kolata, Alan L. (R), 591
 Kolchin, Peter, *The Big Picture: A Comment on David Brion Davis's "Looking at Slavery from Broader Perspectives,"* 467-71
 Kolp, John Gilman, "Gentlemen and Freeholders: Electoral Politics in Colonial Virginia," 539
 Kousser, J. Morgan, "Colorblind Injustice: Minority Voting Rights and the Undoing of the Second Reconstruction," 572
 Kratoska, Paul H., "The Japanese Occupation of Malaya: A Social and Economic History," 526
 Krotov, P. A., "Gangutskaia Bataliia 1714 goda," 644
 Kryzanek, Michael J. (R), 584
 Kuromiya, Hiroaki (C), 679
 "Labor's Great War," by McCartin, 560
Lágrimas Negras, directed by Dolz, reviewed by Davis, 657
 Lambert, Andrew (R), 604
 Land, Jeff, "Active Radio: Pacifica's Brash Experiment," 566
 Landsman, Ned, Wendy F. Katkin, and Andrea Tyree, editors, "Beyond Pluralism: The Conception of Groups and Group Identities in America," 569
 Large, David Clay (R), 635
 Larson, Deborah Welch, "Anatomy of Mistrust: U.S.-Soviet Relations during the Cold War," 522
 Lasar, Matthew, "Pacifica Radio: The Rise of an Alternative Network," 566
 "Lawyers and the Rise of Western Political Liberalism," edited by Halliday and Karpik, 519
 Lees, Andrew (R), 632
 Le Gall, Michel, and Kenneth Perkins, editors, "The Maghrib in Question: Essays in History and Historiography," 516
 LeGrand, Catherine C., Gilbert M. Joseph, and Ricardo D. Salvatore, editors, "Close Encounters of Empire: Writing the Cultural History of U.S.-Latin American Relations," 513
 Lehmann, Hartmut, and Anne-Charlott Trepp, editors, "Im Zeichen der Krise: Religiosität im Europa des 17. Jahrhunderts" (E), 663
 Lembcke, Jerry, "The Spitting Image: Myth, Memory, and the Legacy of Vietnam," 577
 LeMoine, Fannie J., and Christopher Kleinhenz, editors, "Fearful Hope: Approaching the New Millennium" (E), 660
 Lewis, Jan Ellen, and Peter S. Onuf, editors, "Sally Hemings and Thomas Jefferson: History, Memory, and Civic Culture" (E), 662
 Lieberman, Robert C., "Shifting the Color Line: Race and the American Welfare State," 570
 "Life in Mr. Lincoln's Navy," by Ringle, 549
 Lilley, James R., and David Shambaugh, editors, "China's Military Faces the Future" (E), 661
 Lindemann, Albert S. (C), 677
 Lipsitz, George, "The Possessive Investment in Whiteness: How White People Profit from Identity Politics," 573
 Liss, Peggy (C), 676
 "A Little Corner of Freedom," by Weiner, 649
 "The Long Entanglement," by Kaplan, 606
 Longworth, Philip (R), 644
Looking at Slavery from Broader Perspectives, by Davis, 452-66
 Luebke, David M. (R), 630
 Lukowski, Jerzy, "The Partitions of Poland: 1772, 1793, 1795," 641
 Lynn, John A., "Giant of the 'Grand Siècle': The French Army, 1610-1715," 620
- Mackie, Vera, "Creating Socialist Women in Japan: Gender, Labour and Activism, 1900-1937," 525
 Maddux, Thomas R. (R), 564
 "The Maghrib in Question," edited by Le Gall and Perkins, 516
 Magid, Alvin (R), 585
 Major, Patrick, "The Death of the KPD: Communism and Anti-Communism in West Germany, 1945-1956," 639

- "Making American Industry Safe for Democracy," by Haydu, 560
- "Making Early Histories in Museums," edited by Merriman (E), 660
- "The Making of the Mosaic," by Kelley and Trebilcock, 532
- "Making the News," edited by de la Motte and Przybyski, 623
- Malia, Martin, "Russia under Western Eyes: From the Bronze Horseman to the Lenin Mausoleum," 643
- Mansel, Philip, and Kirsty Carpenter, editors, "The French Émigrés in Europe and the Struggle against Revolution, 1789–1814" (E), 663
- Marr, David G. (R), 527
- "Marriage in the Early Republic," by Jabour, 541
- Martínez-Fernández, Luis, "Fighting Slavery in the Caribbean: The Life and Times of a British Family in Nineteenth-Century Havana," 582
- "Mary Baker Eddy," by Gill, 551
- Masters, Bruce (R), 651
- Matsuda, Matt K. (R), 623
- Matthews, Glenna (R), 541
- Matthews, Richard K. (R), 539
- "Maya Conquistador," by Restall, 586
- McCartin, Joseph A., "Labor's Great War: The Struggle for Industrial Democracy and the Origins of Modern American Labor Relations, 1912–1921," 560
- McCauley, Martin (R), 648
- McCoy, Alfred W. (R), 526
- McCreery, David (R), 589
- McDermott, Kevin, and John Morison, editors, "Politics and Society under the Bolsheviks: Selected Papers from the Fifth World Congress of Central and East European Studies, Warsaw, 1995" (E), 664
- McIntosh, Terence (R), 629
- McKibbin, Ross, "Class and Cultures in England, 1918–1951," 613
- McPherson, Kathryn, Cecilia Morgan, and Nancy M. Forestell, editors, "Gendered Pasts: Historical Essays in Femininity and Masculinity in Canada" (E), 661
- "Medicine and Religion c. 1300," by Ziegler, 603
- Medick, Hans, "Weben und Überleben in Laichingen 1650–1900: Lokalgeschichte als allgemeine Geschichte," 629
- Mellars, Paul, John Coles, and Robert Bewley, editors, "World Prehistory: Studies in Memory of Grahame Clark" (E), 660
- Merriman, Nick, editor, "Making Early Histories in Museums" (E), 660
- "Methodism and Education, 1849–1902," by Smith, 611
- Metzner, Paul, "Crescendo of the Virtuoso: Spectacle, Skill, and Self-Promotion in Paris during the Age of Revolution," 622
- "Middlemen of the Cameroons Rivers," by Austen and Derrick, 653
- "Migration and Urbanization in the Ruhr Valley 1821–1914," by Jackson, 632
- Miller, Maureen C. (R), 599
- Minichiello, Sharon A. (R), 525
- Mitchell, Timothy, "Betrayal of the Innocents: Desire, Power, and the Catholic Church in Spain," 617
- Mitchinson, Wendy (R), 509
- "Modern Medea," by Weisenburger, 547
- Modernization from the Other Shore*, by Engerman, 383–416
- Mollenhauer, Daniel, "Auf der Suche nach der 'wahren Republik': Die französischen 'radicaux' in der frühen Dritten Republik (1870–1890)," 624
- Morgan, Cecilia, Kathryn McPherson, and Nancy M. Forestell, editors, "Gendered Pasts: Historical Essays in Femininity and Masculinity in Canada" (E), 661
- Morison, John, and Kevin McDermott, editors, "Politics and Society under the Bolsheviks: Selected Papers from the Fifth World Congress of Central and East European Studies, Warsaw, 1995" (E), 664
- "Morning Glories," by Bridges, 557
- Murphy-Lawless, Jo, "Reading Birth and Death: A History of Obstetric Thinking," 509
- Murray, Alexander, "Suicide in the Middle Ages"; Volume 1, "The Violent against Themselves," 597
- "Museums and American Intellectual Life, 1876–1926," by Conn, 553
- "Muslim Perceptions of Other Religions," edited by Waardenburg (E), 660
- "Nameless Towns," by Sitton and Conrad, 558
- "Der Nationalsozialismus vor Gericht," edited by Uebersch (E), 664
- "Native Americans and the Early Republic," edited by Hoxie, Hoffman, and Albert (E), 662
- "New World Economies," by Egnal, 530
- Ninkovich, Frank (R), 513
- Noel, Thomas J. (R), 512
- Not for Ourselves Alone: The Story of Elizabeth Cady Stanton and Susan B. Anthony*, directed by Burns, reviewed by Fischer, 655
- "La nueva Covadonga insurgente," by Tellería, 618
- O'Connell, Sean, "The Car and British Society: Class, Gender and Motoring, 1896–1939," 614
- "Of Body and Brush," by Zito, 523
- "Oil and Coffee," by De La Pedraja, 580
- Okihiro, Gary Y. (R), 568
- O'Leary, Cecilia Elizabeth (R), 558
- Onuf, Peter S., and Jan Ellen Lewis, editors, "Sally Hemings and Thomas Jefferson: History, Memory, and Civic Culture" (E), 662
- "Opposite Poles," by Erdmans, 579
- Orleck, Annelise (R), 574
- "Otto Neurath," by Cartwright, 637
- "Pacifica Radio," by Lasar, 566
- Padden, Carol A. (R), 616
- "Papacy and Law in the Gregorian Revolution," by Cushing, 599
- Parot, Joseph John (R), 579
- "The Partitions of Poland," by Lukowski, 641
- Patch, Robert W. (R), 586
- Patrias, Carmela (R), 532
- "Patriotic Toil," by Attie, 547
- Pearson, Edward A., editor, "Designs against Charleston: The Trial Record of the Denmark Vesey Slave Conspiracy of 1822," 546
- Pelz, William A. (R), 639
- "People Are Not the Same," by Silla, 653
- "A People Who Would Not Kneel," by Howe, 590

- Perkins, Kenneth, and Michel Le Gall, editors, "The Maghrib in Question: Essays in History and Historiography," 516
- Plann, Susan, "A Silent Minority: Deaf Education in Spain, 1550–1835," 616
- Plummer, Brenda Gayle (R), 555
- "Political Policing," by Huggins, 581
- "Politics and Society under the Bolsheviks," edited by McDermott and Morison (E), 664
- Porter, Roy, and G. S. Rousseau, "Gout: The Patrician Malady," 609
- Portuges, Catherine (R), 656
- "The Possessive Investment in Whiteness," by Lipsitz, 573
- Postel-Vinay, Gilles, "La terre et l'argent: L'agriculture et le crédit en France du XVIII^e au début du XX^e siècle," 621
- "Power and Place in the North American West," edited by White and Findlay (E), 662
- Powers, James F. (R), 601
- "The Presence of the Past," by Rosenzweig and Thelen, 511
- Price, J. L. (R), 627
- Price, Roger (R), 621
- Priester, Peter R., "Geschiedenis van de Zeeuwse landbouw circa 1600–1910," 627
- "Protestantism in Guatemala," by Garrard-Burnett, 589
- Przyblyski, Jeannene, and Dean de la Motte, editors, "Making the News: Modernity and the Mass Press in Nineteenth-Century France," 623
- "Race, Money, and the American Welfare State," by Brown, 570
- Radcliff, Pamela Beth (R), 618
- Ragsdale, Hugh (C), 679
- Ravitch, Norman (C), 678
- Rawnsley, Gary D. (R), 566
- "Reading Birth and Death," by Murphy-Lawless, 509
- "Red Rubber, Bleeding Trees," by Stanfield, 591
- Reilly, Bernard F., "The Kingdom of León-Castilla under King Alfonso VII 1126–1157," 601
- Reimers, David M., "Unwelcome Strangers: American Identity and the Turn Against Immigration," 568
- Reimers, David M. (R), 569
- "Reinventing Russia," by Brudny, 648
- Reis, Elizabeth (R), 537
- "Religions in Conflict," by Copley, 527
- Restall, Matthew, "Maya Conquistador," 586
- Rich, David Alan, "The Tsar's Colonels: Professionalism, Strategy, and Subversion in Late Imperial Russia," 645
- Richardson, William (R), 520
- Richter, Michael, "Ireland and Her Neighbours in the Seventh Century," 594
- Ringle, Dennis J., "Life in Mr. Lincoln's Navy," 549
- "The Rise and Fall of Class in Britain," by Cannadine, 612
- Ritchie, Robert C. (R), 607
- Rodger, N. A. M., "The Safeguard of the Sea: A Naval History of Britain, 660–1649," 607
- Rogow, Arnold A., "A Fatal Friendship: Alexander Hamilton and Aaron Burr," 539
- Rose, Paul Lawrence, "Heisenberg and the Nazi Atomic Bomb Project: A Study in German Culture," 638
- Rosenthal, Joel T., and Martha Carlin, editors, "Food and Eating in Medieval Europe" (E), 662
- Rosenzweig, Roy, and David Thelen, "The Presence of the Past: Popular Uses of History in American Life," 511
- Roshwald, Aviel, and Richard Stites, editors, "European Culture in the Great War: The Arts, Entertainment, and Propaganda, 1914–1918," 604
- Rostworowski de Diez Canseco, María, "History of the Inca Realm," 591
- Rotberg, Robert I., editor, "Creating Peace in Sri Lanka: Civil War and Reconciliation" (E), 661
- Rothney, John (R), 624
- Rousseau, G. S., and Roy Porter, "Gout: The Patrician Malady," 609
- Rubenstein, Anne, "Bad Language, Naked Ladies, and Other Threats to the Nation: A Political History of Comic Books in Mexico," 588
- "The Rule of Law in the Arab World," by Brown, 650
- Rupp, Leila J., "A Desired Past: A Short History of Same-Sex Love in America," 575
- "Russia under Western Eyes," by Malia, 643
- "The Safeguard of the Sea," by Rodger, 607
- "Sally Hemings and Thomas Jefferson," edited by Lewis and Onuf (E), 662
- Salvatore, Ricardo D., Gilbert M. Joseph, and Catherine C. LeGrand, editors, "Close Encounters of Empire: Writing the Cultural History of U.S.-Latin American Relations," 513
- Sander, Kathleen Waters, "The Business of Charity: The Women's Exchange Movement, 1832–1900," 550
- Schiffman, Zachary S. (R), 619
- Schmitt, Jean-Claude, "Ghosts in the Middle Ages: The Living and the Dead in Medieval Society," 598
- Schneid, Frederick C. (R), 620
- Schneider, James C. (R), 577
- Schutte, Anne Jacobson (R), 639
- Schwartz, Stuart B. (R), 592
- Scott, Rebecca J., *Small-Scale Dynamics of Large-Scale Processes*, 472–79
- "Sectional Nationalism," by Sheidley, 545
- Shambaugh, David, and James R. Lilley, editors, "China's Military Faces the Future" (E), 661
- Shannon, Richard, "Gladstone"; Volume 2, "1865–1898," 610
- Sheidley, Harlow W., "Sectional Nationalism: Massachusetts Conservative Leaders and the Transformation of America, 1815–1836," 545
- "Shifting the Color Line," by Lieberman, 570
- Shoemaker, Nancy, "American Indian Population Recovery in the Twentieth Century," 567
- Shorter, Edward (R), 609
- Sievers, Sharon (R), 524
- Silber, Nina (R), 547
- "A Silent Minority," by Plann, 616
- Silla, Eric, "People Are Not the Same: Leprosy and Identity in Twentieth-Century Mali," 653
- Silverstein, Gorder, "Imbalance of Powers: Constitutional Interpretation and the Making of American Foreign Policy," 577
- Siraisi, Nancy G. (R), 603
- Sitton, Thad, and James H. Conrad, "Nameless Towns: Texas Sawmill Communities, 1880–1942," 558

- Slavery at Different Times and Places*, by Engerman, 480–84
- Small-Scale Dynamics of Large-Scale Processes*, by Scott, 472–79
- Smith, Brendan (R), 594
- Smith, Harold L. (R), 613
- Smith, John T., “Methodism and Education, 1849–1902: J. H. Rigg, Romanism, and Wesleyan Schools,” 611
- Soifer, Alexander (R), 656
- Sokurov, Aleksandr, 656
- Sowell, David (R), 591
- Sparks, Randy J. (R), 541
- Spence, Jonathan D., “The Chan’s Great Continent: China in Western Minds,” 523
- Spenser, Daniela, “The Impossible Triangle: Mexico, Soviet Russia, and the United States in the 1920s,” 520
- Sperber, Jonathan (R), 633
- Spierenburg, Pieter (R), 625
- Spinney, Robert G., “World War II in Nashville: Transformation of the Homefront,” 562
- “The Spitting Image,” by Lembcke, 577
- Springer, Kimberly, editor, “Still Lifting, Still Climbing: Contemporary African American Women’s Activism” (E), 662
- “Stalinism in Poland, 1944–1956,” by Kemp-Welch (E), 664
- Stanfield, Michael Edward, “Red Rubber, Bleeding Trees: Violence, Slavery, and Empire in Northwest Amazonia, 1850–1933,” 591
- Stange, Eric (C), 680
- Stevens, Donald F., editor, “Based on a True Story: Latin American History at the Movies,” 515
- Stewart, Gordon T. (R), 531
- “Still Lifting, Still Climbing,” edited by Springer (E), 662
- Stine, Jeffrey K. (R), 562
- Stites, Richard, and Aviel Roshwald, editors, “European Culture in the Great War: The Arts, Entertainment, and Propaganda, 1914–1918,” 604
- Stone, Daniel (R), 641
- “The Struggle over the Soul of Economics,” by Yonay, 559
- Sturken, Marita (R), 577
- “The Style of Paris,” by Huppert, 619
- “Suicide in the Middle Ages”; Volume 1, “The Violent against Themselves,” by Murray, 597
- Sunshine [A Napfény íze]*, directed by Szabó, reviewed by Portuges, 656
- “Suomen syntä,” by Alapuro, 626
- Szabó, István, 656
- “Taking Heaven by Storm,” by Wigger, 541
- Tarling, Nicholas, “Britain, Southeast Asia and the Onset of the Cold War, 1945–1950,” 521
- Taylor, Sandra C., “Vietnamese Women at War: Fighting for Ho Chi Minh and the Revolution,” 527
- Tellería, Javier Ugarte, “La nueva Covadonga insurgente: Orígenes sociales y culturales de la sublevación de 1936 en Navarra y el País Vasco,” 618
- Tengwall, David (R), 580
- Tenorio, Mauricio (R), 588
- “La terre et l’argent,” by Postel-Vinay, 621
- Thelen, David, and Roy Rosenzweig, “The Presence of the Past: Popular Uses of History in American Life,” 511
- “Theory of History in Ortega y Gasset,” by Graham, 510
- Thinking Unfashionable Thoughts, Asking Unfashionable Questions*, by Jacob, 494–500
- A Time of Reconquest*, by Benjamin, 417–50
- “To Believe in Women,” by Faderman, 575
- “Trade in Strangers,” by Wokeck, 534
- Trafzer, Clifford E. (R), 544
- Trebilcock, Michael, and Ninette Kelley, “The Making of the Mosaic: A History of Canadian Immigration Policy,” 532
- Trepp, Anne-Charlott, and Hartmut Lehmann, editors, “Im Zeichen der Krise: Religiosität im Europa des 17. Jahrhunderts” (E), 663
- “The Triumph of Ethnic Progressivism,” by Connolly, 556
- “The Tsar’s Colonels,” by Rich, 645
- Tushnet, Mark (R), 572
- Tyree, Andrea, Wendy F. Katkin, and Ned Landsman, editors, “Beyond Pluralism: The Conception of Groups and Group Identities in America,” 569
- Tyson, Timothy B., and David S. Cecelski, editors, “Democracy Betrayed: The Wilmington Race Riot of 1898 and Its Legacy,” 554
- Uebersch, Gerd R., editor, “Der Nationalsozialismus vor Gericht: Die alliierten Prozesse gegen Kriegsverbrecher und Soldaten 1943–1952” (E), 664
- “Under the Hammer,” by Watson, 602
- “Unwelcome Strangers: American Identity and the Turn against Immigration,” by Reimers, 568
- Vansina, Jan (R), 653
- “Verkehrte Welt,” by Geyer, 635
- Vicinus, Martha (R), 575
- “Vietnamese Women at War,” by Taylor, 527
- Villalon, L. J., and Donald J. Kagay, “The Final Argument: The Imprint of Violence on Society in Late Medieval and Early Modern Europe,” 595
- “Violence and Society in the Early Medieval West,” edited by Halsall, 595
- Viswanathan, Gauri (R), 527
- Waardenburg, Jacques, editor, “Muslim Perceptions of Other Religions: A Historical Survey” (E), 660
- Wallace, Mike, and Edwin G. Burrows, “Gotham: A History of New York City to 1898,” 536
- Walthall, Anne, “The Weak Body of a Useless Woman: Matsuo Taseko and the Meiji Restoration,” 524
- Warren, Leonard, “Joseph Leidy: The Last Man Who Knew Everything,” 552
- Watson, Fiona J., “Under the Hammer: Edward I and Scotland, 1286–1307,” 602
- “The Weak Body of a Useless Woman,” by Walthall, 524
- “Weben und Überleben in Laichingen 1650–1900,” by Medick, 629
- Weiner, Douglas R. (R), 646
- Weiner, Douglas R., “A Little Corner of Freedom:

- Russian Nature Protection from Stalin to Gorbachëv," 649
- Weisenburger, Steven, "Modern Medea: A Family Story of Slavery and Child-Murder from the Old South," 547
- Wenders, Wim, 657
- "Western Warfare in the Age of the Crusades 1000–1300," by France, 596
- Westwood, J. N. (R), 647
- Whelan, Christopher T., Anthony F. Heath, and Richard Breen, editors, "Ireland North and South: Perspectives from Social Science" (E), 663
- White, Richard, and John M. Findlay, editors, "Power and Place in the North American West" (E), 662
- Whose Measure of Reality?* by Goldstone, 501–08
- Widder, Keith R., "Battle for the Soul: Métis Children Encounter Evangelical Protestants at Mackinaw Mission, 1823–1837," 543
- Wigger, John H., "Taking Heaven by Storm: Methodism and the Rise of Popular Christianity in America," 541
- Wilson, Carol (R), 547
- Wilson, Larman C., and G. Pope Atkins, "The Dominican Republic and the United States: From Imperialism to Transnationalism," 584
- Wilson, Lisa, "Ye Heart of a Man: The Domestic Life of Men in Colonial New England," 537
- Wokeck, Marianne S., "Trade in Strangers: The Beginnings of Mass Migration to North America," 534
- Woods, Randall B. (R), 522
- "World Prehistory," edited by Coles, Bewley, and Mellars (E), 660
- "World War II in Nashville," by Spinney, 562
- "The Worm in the Wheat," by Henderson, 586
- Yang, Anand A., "Bazaar India: Markets, Society, and the Colonial State in Gangetic Bihar," 529
- "Ye Heart of a Man," by Wilson, 537
- Yonay, Yuval P., "The Struggle over the Soul of Economics: Institutional and Neoclassical Economists in America between the Wars," 559
- Yorke, Barbara (R), 595
- "The Young Colonials," by Campbell, 585
- Ziegler, Joseph, "Medicine and Religion c. 1300: The Case of Arnau de Vilanova," 603
- Zimmerman, Jonathan, "Distilling Democracy: Alcohol Education in America's Public Schools, 1880–1925," 551
- Zito, Angela, "Of Body and Brush: Grand Sacrifice as Text/Performance in Eighteenth-Century China," 523
- Zolov, Eric (R), 515
- Zuccotti, Susan (R), 640
- "Zwischen Markt und Profession," by Franz, 634



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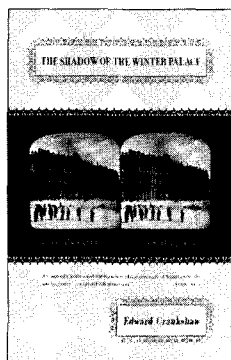
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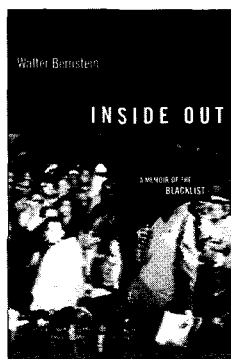
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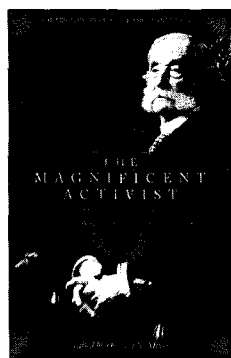
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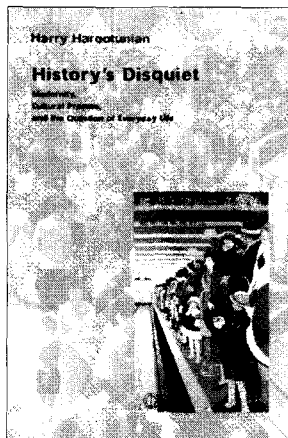
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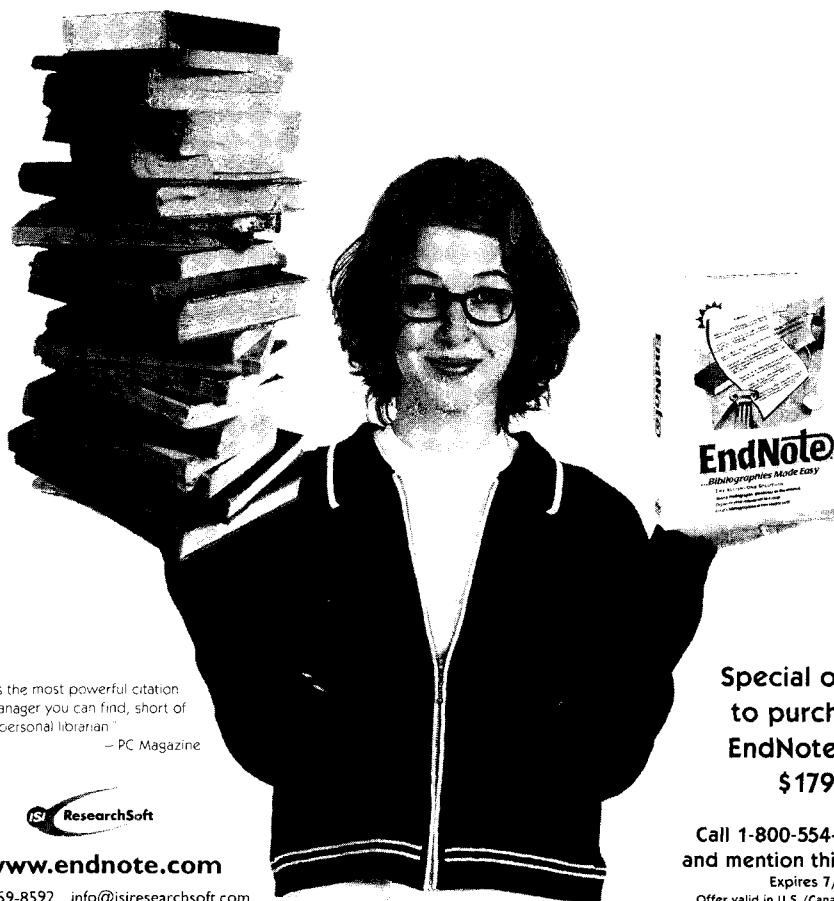
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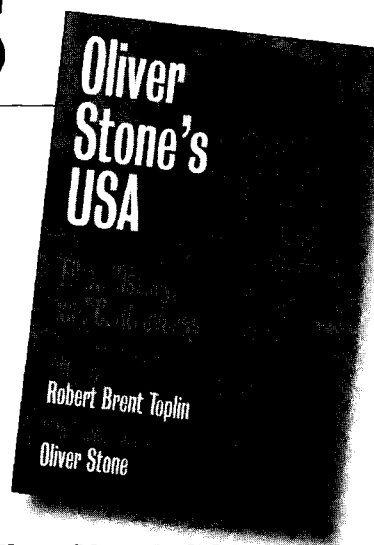
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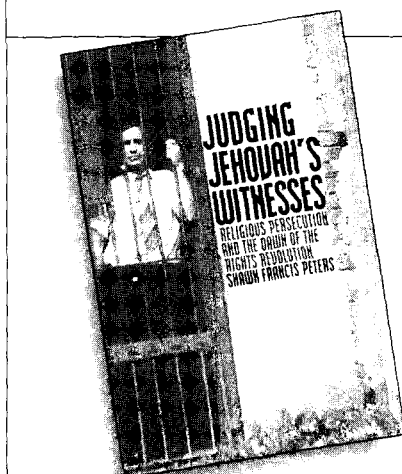
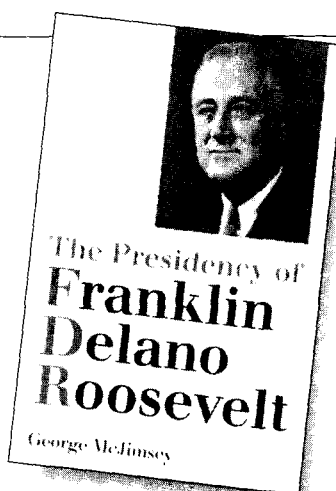
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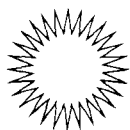
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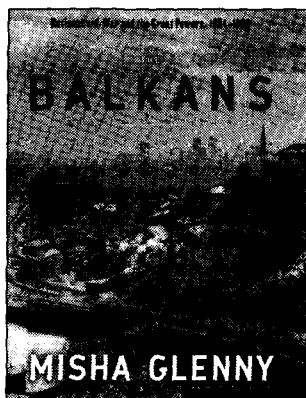
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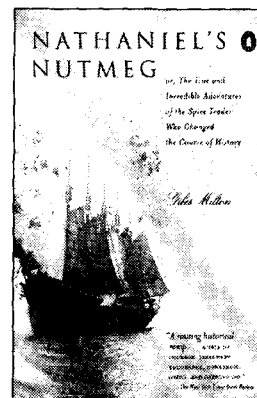
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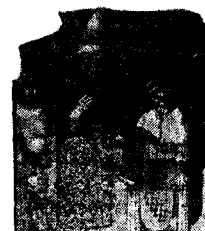
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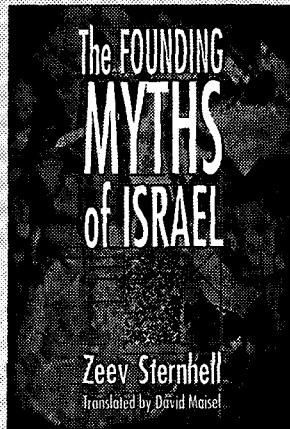
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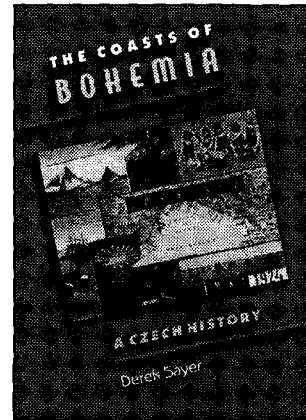
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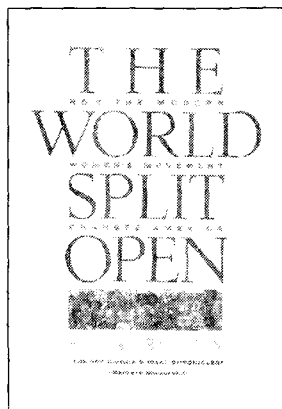
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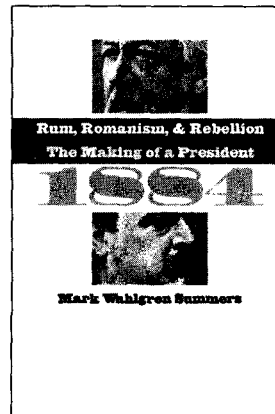
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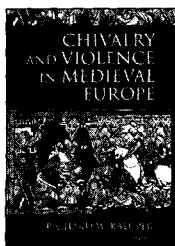
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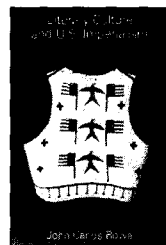
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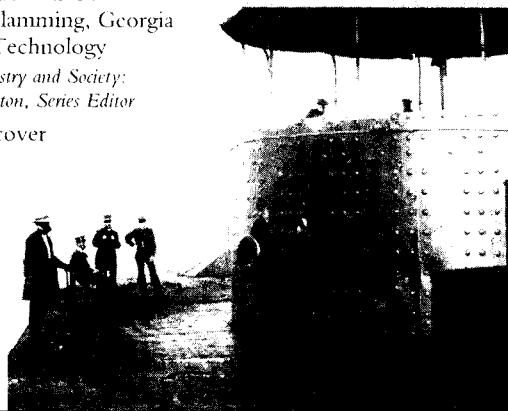
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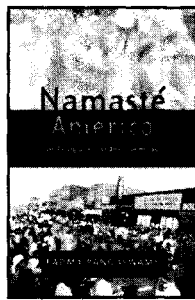
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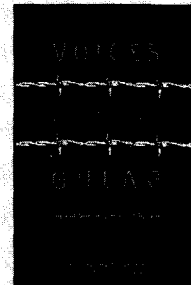
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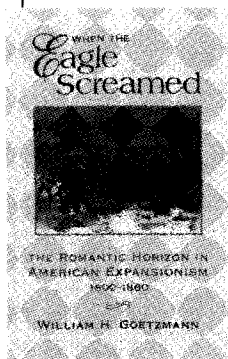
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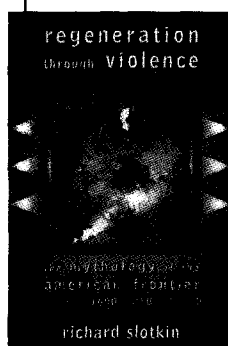
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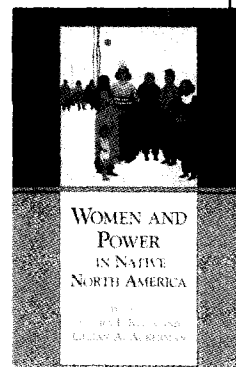
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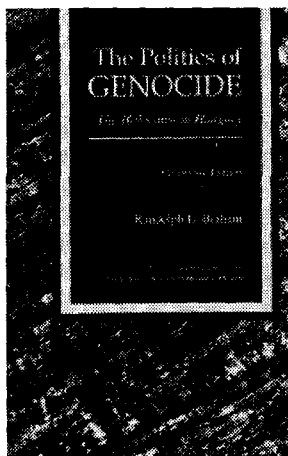
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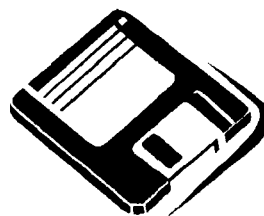
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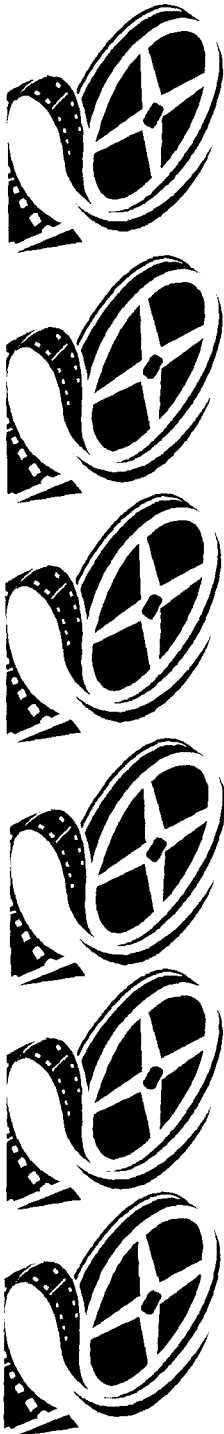
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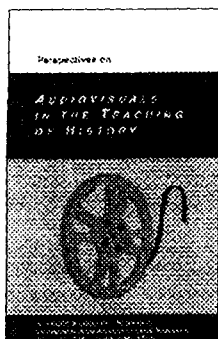
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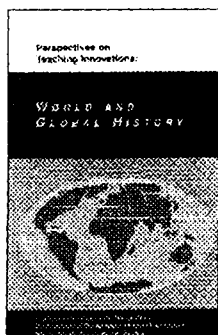
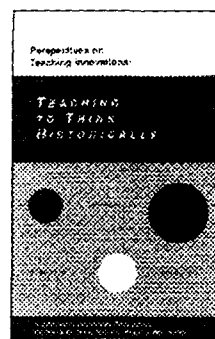


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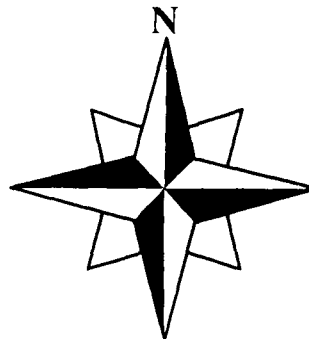
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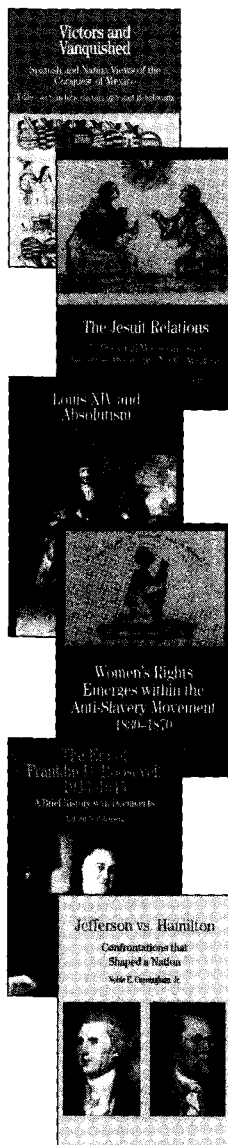
Index of Advertisers

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Da Capo Press	8	Southern Illinois University Press	38
Duke University press	33	University of California Press	9–11
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